THURSDAY, OCTOBER 28, 1880

BALFOUR'S "COMPARATIVE EMBRYOLOGY"

A Treatise on Comparative Embryology. By Francis M. Balfour, M.A., F.R.S., Fellow and Lecturer of Trinity College, Cambridge. In Two Volumes. Vol. I. (London: Macmillan and Co., 1880.)

IT is scarcely possible to exaggerate the expressions of gratitude which are due from zoologists to Mr. Balfour for the execution of the great task which some three or four years ago he set himself. Zoologists have to be thankful to him not only for the admirable style in which he has carried out his work, but for the promptitude with which he has achieved it. Mr. Balfour's object was to produce a work in which all that has been written during the last ten or fifteen years on the structural features exhibited by animals during their growth from the egg to the adult condition should be digested, and its import carefully estimated; the result being set forth in a systematic way, so that the broad conclusions arrived at by the almost innumerable studies of "development from the egg" in all sorts and conditions of animals should be pointedly placed before the reader. At the same time he aimed to provide for the purpose of reference and for the guidance of future students something like a complete bibliography, accompanied by an analysis in many cases, of the works which have been published on special forms.

It is very well known to those who are in a position to make a comparative estimate, that during the last fifteen years in no branch of science has there been such activity, such abundance of discovery, of careful observation and ingenious speculation, as in biology; and this activity has tended more and more to concentrate itself upon the study of the mode in which the complex adult organism (whether plant or animal), with all its astounding powers and its beauty of form—slowly, surely, and yet by most improbable and devious ways, advances to its complete estate from the condition of a microscopic structureless globule of albuminous slime. This marvel of development is one which has only recently come to man's knowledge, and it seems likely that the fascination which the study of it can exert will be such as to attract the energies of an ever-increasing crowd of observers.

Mr. Balfour's book gives for those who are to come a *résumé* or summing up of the labours of those who have up to this date worked for and created our knowledge of what this process of growth from the egg is and signifies.

The first volume deals with the history of development in all groups of animals excepting the Vertebrata. The labour which it has involved will be understood when it is stated that the author gives references to five hundred and seventy-two separate memoirs or books, most of which he has thoroughly read, and from many of which he gives extracts or carefully condensed abstracts.

The thoroughness with which the subject is presented to the student may be appreciated by a consideration of the fact that two hundred and seventy-five woodcuts are given in this volume, which are, with few exceptions, prepared especially for this work, either from the author's original drawings or from the drawings of the writers whom he is summarising.

The work is divided into an "Introduction" and a "Systematic Embryology." In the Introduction we have chapters on "The Ovum and the Spermatozoon," on "The Maturation and the Impregnation of the Ovum," and on "The Segmentation of the Ovum." The systematic portion is divided into chapters, each of which corresponds with one of the large divisions of animals, *e.g.* Porifera, Platyelminthes, Rotifera, Mollusca, Chætopoda, &c.

Mr. Balfour, it is hardly necessary to say, has not performed his task as an ordinary maker of books. He is, as all zoologists know, one of the foremost students of embryology in Europe, and has added a very large proportion himself to that great heap of isolated embryological memoirs and monographs which it is the purpose of his book to condense and render accessible to a wider circle of students. Consequently we find not only new and original observations scattered here and there in the chapters of this treatise, but on the very numerous matters which call for the expression of an opinion or the exercise of judgment between conflicting statements of preceding observers, we have the conclusions, always modestly formulated, of a thoroughly competent critic.

In fact those who are already advanced in the study of embryology will find that Mr. Balfour has freely and most legitimately made use of speculative views of his own, as a series of strings on which to thread the almost innumerable observed facts which have to be put on record and kept ready, as it were, for the future building up of embryological doctrine. The reader, on the other hand, who has not yet reached the degree of knowledge at which such speculations become intelligible, will find that there is so much in Mr. Balfour's pages of hard, solid, descriptive record of the actual developmental changes of one animal after another, that he will certainly not feel cause to complain.

It would be out of place to discuss in these pages any of the new theoretical considerations which Mr. Balfour puts forward. With some of them it is possible to find fault; at the same time they are all ingeniously supported and indicate close reasoning and a large survey of facts on the author's part. They serve, as Mr. Balfour himself recognises, to stimulate inquiry, and when advanced not by a paper-philosopher, but by a most exceptionally industrious observer, they cannot fail to command respect.

If we venture to offer any remark which suggests how possibly Mr. Balfour's book might have been even more excellent than it is, it must be clearly understood that as it stands we hold it to be a perfect mine of valuable information and well-considered suggestion. We should, however, have been glad had it been possible for the author to give more attention to the history of the various stages of progress in our knowledge of embryology in general, and of each particular group. Full justice is done to recent authors, and his own contemporaries receive ample recognition from Mr. Balfour; but the successive steps by which a particular point of view has been arrived at are not always definitely indicated and due merit assigned to each of those who in past times has laboured to bring about the present phase of science. This, no doubt, has not entered into Mr. Balfour's plan on account of the additional responsibility and labour which it would have involved, and the increase in size of what is already

a voluminous treatise. But such treatment of the subject has a very high educational value and a certain ethical importance.

Further, it may be noted that the author has necessarily a difficulty to contend with in the scope itself of his book. Embryology is not a natural nor a convenient division of biological science. The study of the organism in its complete form cannot be advantageously separated from the study of the coming about of that form, and indeed it is very difficult for a writer who proposes to himself to describe the developmental changes of organisms to draw the line consistently in the various cases which he describes, and to say that at such a point his business with the organism ceases and that of the "antipædologist" begins. It is because the knowledge of embryological facts is to so large an extent new, that separate treatises on embryology are necessary. It is as a supplement to treatises on the structure or anatomy of animals which do not sufficiently deal with embryology that such a distinct treatise is needful, and such need is merely the result of the late development of embryological research.

In the course of time we shall no doubt see a complete fusion of "embryology" and "antipædology"—the facts of structure to be observed in the youth and in the maturity of organisms being treated as a matter of course concurrently. Nothing could conduce more directly to this desirable state of things than the really remarkable and successful effort which Mr. Balfour has made to gather together and present in a compact and logical form the embryological results which have been and still are pouring forth from Russian, German, English, French, and American laboratories in an overwhelming stream, calculated to daunt by its velocity any but the most determined student. E. RAY LANKESTER

THE SIEVE-TUBES OF DICOTYLEDONOUS PLANTS

Beiträge zur Kenntniss des Siebröhrenapparates dicotyler Pflanzen. Von Dr. Karl Wilhelm. (Leipzig: W. Engelmann, 1880.)

IT is perhaps natural, owing to its peculiarities, and especially to the character of the cell walls, that the soft bast was comparatively lately investigated and described;[1] but it is surely a surprising fact that the ground should have been left open till the present year, for a thorough investigation of the development of those tissues which are characteristic of the phloem.

In the "Comparative Anatomy" of De Bary we find a full account of what was known in 1877 of the structure and development of the soft bast; at the same time the writer pointed out several questions concerning which further investigation was required. He drew especial attention to our want of knowledge of the relation of the cambiform cells[2] to the sieve-tubes, and of the development of the sieve plate, the callus mass, and the contents of the sieve-tube. It has been the object of Dr. Wilhelm's researches to supply information on these several points; while at the same time he affords us many other interesting details.

Owing to the wideness of the subject it was impossible for the author to extend his researches beyond a limited number of types. Those selected were *Vitis vinifera*, L., *Curcubita pepo*, L., and *Lagenaria vulgaris*, Ser. It will be seen that Dr. Wilhelm has selected plants having sieve-tubes of the two different types common among the Dicotyledons, viz., Cucurbita and Lagenaria where the structure is more simple, Vitis where it is complicated by the presence of several sieve-plates side by side on the same cell wall. In a note at the end of the paper the author specially asserts that his results only apply to the plants named; while further research must show whether the structure described is really typical.

The main results arrived at are as follows:—Those formative cells of the bast which are set apart for the development of a member of a sieve-tube, usually suffer a longitudinal division into two unequal cells: the larger forms one member of the sieve-tube; the other, which is smaller and shorter, develops into the companion-cell (*Geleitzelle*). The latter may, in Cucurbita and Lagenaria, again divide. The walls separating the companion-cells from the sieve-tube are fitted, and the cell contents richly protoplasmic. It will be seen that these cells, being sister cells of the members of the sieve-tubes, must be distinguished from the larger cells, which are usually termed "cambiform;" these latter being formed by division from formative cells of the bast, but not being in direct genetic connection with the cells, which develop into members of the sieve-tubes.

Dr. Wilhelm finds that the "callous" condition of the sieve-plate is not, as previously supposed, the result of a secondary change of the plate; on the contrary, the differentiation of the sieve-plate begins by the change of the cellulose to "callus" at a number of points. It is in the callus masses, formed at these points, that the pores of the sieve later appear. The callus may extend itself from these points so as to cover the whole face of the plate, and completely inclose the cellulose sieve. A callus-skeleton is thus formed which may be isolated.

The callus varies in volume, increasing with age, or on approach of the period of rest; in which case the pores may be completely stopped; or decreasing as the period of summer activity approaches, when the pores are again opened. This result may be obtained by artificial means. It is best seen in Vitis; it is probable that this variation of volume of the callus is by no means universal.

As regards the substance of the callus it will be seen from the following reactions that it cannot be identified with any of the substances previously described. With acids and alkalies it swells quickly; if the reagents be strong it is dissolved. Ammoniacal sub-oxide of copper attacks it only slightly, or not at all; by use of this reagent the callus-skeletons before mentioned may be obtained free. Solution of iodine in alcohol does not colour it; solution of iodine in potassium iodide colours it yellow to brownish yellow. This with Schultz's solution gives a deep red brown; when used alone the latter reagent gives no colour, but causes considerable swelling.

Thus far we have only discussed the cell walls. While the development of the sieve has been going on, but before the perforations are formed, a change appears in

[1] The sieve tubes were discovered by Hartig (1837). His observations were many years after verified by other observers, especially von Mohl, Nägeli, and Hanstein.

[2] De Bary, "Vergl. Anat.," p. 337.

the contents of the young sieve-tubes. Isolated drops or irregular masses appear in the layer of protoplasm lining the cell cavity before the disappearance of the nucleus. These consist of a slimy stuff (*Schleim*) apparently rich in nitrogen.[1] The separate masses later fuse together to form a band, which is usually much narrower than the girth of the cell. Between this and the wall of the sieve tube a protoplasmic envelope intervenes (*Hüllschlauch*). The central cavity within these is filled with "sieve-tube sap." For further details concerning the contents of the sieve-tubes the reader must be referred to the original work.

The author has not been able to observe directly the first appearance of connection through the pores of the sieve; but suggests that it is effected by the outgrowth of protuberances of the envelope (*Hüllschlauch*) from opposite sides of the sieve, which penetrate it and coalesce to form the connecting strings.

The presence of starch grains noticed by Briosi is confirmed by Wilhelm in Vitis. He finds them in members of sieve-tubes which are still closed. He opposes the idea that they pass through the sieve on ground of their size. In Cucurbita and Lagenaria they are absent. Besides the communication of sieve-tubes with one another laterally, so as to form a complete system, Dr. Wilhelm has observed in the case of Vitis a further connection, through the medullary rays, of tubes lying on opposite sides of the ray. This is effected by special sieve tubes, produced by transformation of cells of the medullary ray, so as to form a series of very short members; these correspond in development and structure with the ordinary sieve-tube. They traverse the medullary rays in an obliquely tangential direction. Such communications are not found in Cucurbita or Lagenaria.

The question of function has not been solved by these observations. Dr. Wilhelm still holds the view, propounded by Nägeli, that the function of the sieve-tube is the transference of indiffusible substances from place to place in the plant.

In conclusion it may be remarked that the paper is well written, but that it is of such a character as to be interesting only to the specialist. The plates, of which there are nine, are executed with great skill and exactitude.

F. ORPEN BOWER

OUR BOOK SHELF

The Elementary Geometry of Conics. By C. Taylor, M.A. Third Edition. (Cambridge: Deighton, 1880.)

MR. TAYLOR has been before the public as a writer on geometrical conics since 1863, in which year he brought out his "Geometrical Conics"; in 1872 we have the first edition, and in 1873 the second edition of his "The Geometry of Conics," a smaller work than his first book (1863). Now we have a third edition with the above title. In May, 1875, Mr. Taylor, in a paper entitled "On the Method of Reversion applied to the Transformation of Angles" (read before the Mathematical Society, and subsequently printed in a more extended form in the *Quarterly Journal*, No. 53, 1875, with the title "The Homographic Transformation of Angles"), called attention to a "neglected work on conics by G. Walker, F.R.S. (1794)": in this work we first meet with the properties of a circle, which Walker calls the *generating circle*, but which Mr. Taylor, in the work before us, styles the *eccentric circle*; in the free use of this circle consists the main feature in the alterations made in this new edition; further, though still keeping well in view the proving chord-properties independently of tangent-properties, there is a rearrangement of the text; so that the two properties are not treated of in distinct chapters. In other ways also we think this little work is improved, but we need say no more upon a third edition.

[1] Cf. De Bary, "Vergl. Anat.," p. 185.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Ceraski's New Variable Star

UNLESS the principal fact mentioned below has already come to your notice, you may like to bring it before the astronomical public in the columns of NATURE.

The true period of the variable star recently discovered at Moscow (*Durchmusterung*, zone + 81°, No. 25) appears to be two days and a half, instead of five as given in NATURE, vol. xxii. p. 455. Minima were observed at the Harvard College Observatory on September 23 and 28. The changes of the star will accordingly be visible in England on October 13, 18, 23, 28, &c., during the three or four hours before or after midnight. The rapidity of the change is probably greater in the case of this star than in that of any other known variable, the variation exceeding a magnitude in the course of one hour. The total variation is more than two magnitudes. A star of about the eighth magnitude (No. 30 of the same zone) is within a few minutes of the variable, and may readily be compared with it. The phenomenon of the variation is consequently a striking one, even as seen in a small telescope. The approximate place of the variable for 1881 is in R.A. 0h. 51m. 48s., Decl. +81° 14′·1.

EDMUND C. PICKERING

Harvard College Observatory, Cambridge, U.S., October 2

LORD LINDSAY'S Dun Echt Circular, No. 10, which I received on Saturday morning, October 23, prepared me to watch for a probable minimum of M. Ceraski's remarkable variable star B.D. + 81° 25′ on the same night. From my observations the minimum appears to have occurred at about 11h. 10m. G.M.T., the star then being of about 9·1 magnitude. At 9h. 5m. I noted it about equal to a neighbouring star, B.D. + 81° 30′, which I gauged 8·1 mag., and at 13h. 50m. it had regained the same magnitude. When about minimum I thought the variable to be slightly ruddy, but as it brightened up again it lost this tint and appeared to be white, or bluish white, as when I first observed it. It has a small bluish 11½ mag. companion, the P. and D. of which I roughly estimated to be 60° and 10″ respectively. The star was observed by Carrington in 1855, on December 19, 21, and 30, his estimated mags. being 8·0, 8·0, 9·0. Possibly the star may have been near minimum at his third epoch.

Knowles Lodge, Cuckfield, October 25 GEORGE KNOTT

"Solid Ice at High Temperatures"

THE interesting results announced by Prof. Thomas Carnelley, of Firth College, Sheffield, in relation to the physical conditions under which *ice* persistently maintains its *solid state* when exposed to the influence of heat (NATURE, vol. xxii. p. 435), deserves some notice. When he speaks of obtaining "solid ice at temperatures so high that it was impossible to touch it without *burning* one's self," it is evident that this *burning quality appertains to the hot vessel containing the ice, and not to the solid ice itself.* For it is obvious that under the given conditions *the temperature of the surface of the ice is kept at least as low as* 0° C. *by the rapid vaporisation of it while in a solid state.*

The phenomenon of a body remaining persistently at a low temperature when surrounded by a hot vessel—through the influence of the rapid change of state—is analogous to the well-known results of Boutigny and Faraday in relation to the freezing of water and mercury in a hot vessel by means of large

globules of sulphurous acid and liquid carbonic acid while in a "spheroidal condition." In these cases, notwithstanding the proximity of the hot vessel, the temperatures of the globules of SO_2 and CO_2 are respectively as low as $-10°$ and $-73°$ C.

It has long been remarked by physicists that some substances pass *directly* from the *solid* to the *gaseous* state, without undergoing liquefaction: that is, when heated, *they sublime without melting*. Such bodies, under ordinary atmospheric pressures, have their *boiling* points *lower* than their temperatures of *fusion*; hence they *volatilise* without *melting*. Moreover it has long been known that such substances may be made to *fuse* by subjecting them to an *abnormal pressure* sufficient to *raise* their boiling points *above* their points of fusion. Thus the classical experiments of Sir James Hall show that carbonate of lime may be *fused* when heated under a pressure sufficient to prevent the CO_2 from escaping (*Trans.* Royal Soc. Edin., vol. vi. pp. 71-186, 1805). In like manner metallic arsenic sublimes without melting at 180° C., under the ordinary pressure of the atmosphere; but the experiments of Landolt in 1859 show that under artificial pressure it melts in globules at a low red heat. It is evident that in these cases the *rapid vaporisation* of the solids under ordinary circumstances *prevents* the temperature from reaching the point of fusion; but when subjected to additional pressure the conditions of liquefaction are secured. On the other hand, in the case of *ice*, it is obvious that the *withdrawal of pressure* by *lowering* its boiling-point places it in the same category with metallic arsenic under ordinary conditions of pressure.

In relation to the *literature* of this subject it is proper to add the following quotations from M. V. Regnault's "Elements of Chemistry" (American Translation, Philadadelphia, 1865, vol. i. p.279). In speaking of the fusion of metallic arsenic under pressure he says:—"The distance between the point of fusion and that of ebullition of any body may, however, be increased at pleasure. For *the point of ebullition of a body is the temperature at which the tension of its vapour is equal to the pressure exerted upon it*; and hence by increasing the pressure the boiling-point is raised without sensibly affecting the point of fusion." Again, he says:—"Reciprocally it is evident that a *volatile solid body* may be always subjected to so slight a pressure that it will *boil* at a temperature *inferior* to that at which it *melts*. Thus ice at the temperature of $-1°$ C. possesses an elastic force represented by 4·27 mm.; in other words, it boils at a temperature of $-1°$ C. under the pressure of 4·27 mm. Ice may therefore be entirely volatilised *by ebullition* under this feeble pressure, without reaching its point of fusion, which is 0° C."

Berkeley, California, September 30 JOHN LECONTE

Wire Torsion

THE phenomena described by Major Herschel in his letter to NATURE, vol. xxii. p. 557, and about which he asks for information, are, we think, quite easily explained by what is known of the fluidity of metals. Yielding, or flowing, seems to occur in all metals after a certain limiting stress has been reached; indeed it probably occurs, although perhaps to an immeasurably small extent, even with small stresses (see *Proc.* Roy. Soc. No. 204, p. 411, 1880); but there is generally a limiting stress beyond which the increase of strain due to yielding becomes comparable in magnitude with the ordinary strains, which instantaneously disappear on the removal of the load. The bell-smith pulls his copper wire, and makes it much longer before he thinks it fit for use; in a similar way the telegraph constructor stretches, or *kills* the iron wire before he erects the line. Up to a certain limit of pulling force, the wire obeys the well-known laws of elasticity; slightly above that limit there is considerable fluid-yielding, there being but very little yielding below that limit; and at any instant during the lengthening if the man ceases to pull, the wire shortens a little. In fact at any stage the wire obeys the elastic law for *small* stresses. Eventually the man ceases to pull, knowing that the metal has lost most of its fluid properties, which can only be restored to it by annealing. The same thing occurs in brass, although to a smaller extent than in copper, which can be experimentally proved in the following way:—Stretch a piece of well-annealed brass wire in the manner described by Major Herschel until it is nearly breaking; and immediately set the wire vibrating. Now the note given out by the stretched brass wire, which, as is well known, depends on the tensile stress, will be found rapidly to go down in pitch. If the wire be tightened up again sufficiently with the screw, the original note will again be heard, and the pitch will again go down, but not so rapidly as before. Repeat this process until no flattening of the note is heard; then in this state we think that the experimenter will find the wire to break with much less torsion than before, and to obey Hooke's law more exactly. If it be desired to repeat the yielding or flowing process, the wire must be previously again annealed.

Mere sudden straining, even nearly up to the breaking stress, is not sufficient to destroy the fluidity of brass; time is required. The yielding behaviour of a brass beam when loaded has been studied by Prof. Thurston (*Trans.* American Soc. of Civil Eng. vol. vi. p. 28), and we may add that we have found that the permanent state is always more rapidly reached when the wire is subjected to rapid vibrations.

It may be because torsion of a wire is more visible than longitudinal strains (the twist being inversely proportional to the fourth power of the diameter for a given twisting moment, whereas the longitudinal strain for a given load is inversely proportional to the square of the diameter) that fluidity is so much more apparent in torsional experiments; but we think it probable that fluidity will be found always much more apparent when the volume of the material acted on is unchanged, that is, when the stress is mainly one of shear as it is in torsion.

However this may be we can explain why wire which has been "killed" for pulling forces is not "killed" for twisting, and why it is more difficult to kill for twisting than for tensile stresses. It is well known to wire-drawers that in whatever state copper or brass wire may be, whether annealed or not, it may be drawn smaller, although no doubt it requires less care to draw it if it is annealed. We cannot merely pull wire much smaller, it has to undergo a lateral pressure such as the die gives it. Now in twisting a wire it everywhere receives this lateral pressure, that is—imagine a right-handed spiral filament being lengthened by the twist—then the other component of the twist gives to the filament a compression at right angles to its length which enables it to extend. It seems that this lateral pressure is needed to overcome some sort of friction in the particles of the metal tending to prevent their moving into the axis of the wire, and which therefore is greater as the section of the wire is larger, and it is probably for this reason that a very thin wire extends much more, for a given initial length, before it is killed than a thick wire. We have known a length of about fifteen inches of fine copper wire which had just been drawn, and which had been well killed, to bear six or seven hundred complete turns in a lathe, one end being fixed, the other end turned, and the wire kept pretty taut before it was accidentally broken, and even afterwards parts of the wire could be considerably lengthened by pulling. The nature of the explanation of this apparent annealing for tensile stresses arising from previous torsion will be gathered from what follows.

We infer that the three or four turns given to the wire at the beginning in Major Herschel's experiment were not sufficient to produce permanent torsional set; why then should increasing the tension during the torsion cause torsional set as well as lengthening of the wire? This is, we think, a more important question than the one presented to us by the observations of fluidity in the latter half of Major Herschel's letter, and which arose from the metal having belonged to what Prof. Thurston calls the "tin class" as distinguished from metals of the "iron class."

The explanation we think is as follows, and it leads to the conclusion that torsional fluidity is not independent of tensile stress:—

Suppose right- and left-handed spirals had been imagined in the wire in question, making everywhere angles of 45° with the axis of the wire; then torsional strain, however set up, would consist in the production of a difference in length of these two sets of spirals. Now a twisting moment produces this effect; it lengthens, say the right-hand spiral and shortens the left, and we know that up to a certain limit, which is tolerably high, the same effect is produced whatever be the tensile stress in the wire, which latter simply tends to lengthen both spirals equally. In fact if Hooke's law is true, the torsion is independent of the tension. But above a certain limit of pull in the wire, the strain in the direction of the right-handed spiral being everywhere due to the sum of two tensile stresses, becomes so great that fluidity sets in and permanent set is produced; whereas in the direction of the left-handed spiral the stress is due to the difference between the tensile stress and the compressive part of the torsional shearing stress, and this difference being small, no permanent tensile set is produced, or at all events one much less than in the case of the other spiral. Consequently if all stresses now cease to act

a permanent difference would remain in the lengths of the two spirals, that is, there would now be a permanent twist.

Information regarding the fluidity of tempered steel, copper, brass, lead, tin, &c., will be found in the papers of M. Tresca, and in the second of the Cantor lectures delivered by Mr. Anderson before the Society of Arts April 19, 1869, as well as in Mr. Anderson's book on the "Strength of Materials," and in Mr. Bottomley's reports communicated at the Meetings of the British Association in 1879-80. We do not think, however, that much of the valuable information on the fluidity of metals which is scattered through the *Proceedings* of the different societies has yet been collated. Wire-drawers, watch and clockmakers, as well as the makers of philosophical instruments and of other small machinery, have a considerable amount of knowledge of this subject which they cannot systematise and make known to others, but which, nevertheless, they make ready use of in their work.

Finally, we would suggest that if Major Herschel wants his wire to obey Hooke's law for small twists only, he will not find it necessary to destroy the properties which are due to its being annealed. If, however, he desires to use greater twists, it will be necessary to leave the wire under a fairly large pull for a considerable time without twisting it until it ceases to continuously yield to tensile stresses of greater intensity than that of the shear stress to which it has afterwards to be subjected. And if in Mr. Allan Broun's gravimeter it be necessary to employ such large twisting couples as Major Herschel was using in his experiments, we would suggest the employment of a longer and thicker wire.

JOHN PERRY
W. E. AYRTON

London, October 18

On the Skin-furrows of the Hand

IN looking over some specimens of "prehistoric" pottery found in Japan I was led, about a year ago, to give some attention to the character of certain finger-marks which had been made on them while the clay was still soft. Unfortunately all of those which happened to come into my possession were too vague and ill-defined to be of much use, but a comparison of such finger-tip impressions made in recent pottery led me to observe the characters of the skin-furrows in human fingers generally. From these I passed to the study of the finger-tips of monkeys, and found at once that they presented very close analogies to those of human beings. I have here few opportunities of prosecuting the latter study to much advantage, but hope to present such results as I may attain in another letter. Meanwhile I would venture to suggest to others more favourably situated the careful study of the lemurs, &c., in this connection, as an additional means of throwing light on their interesting genetic relations.

A large number of nature-prints have been taken by me from the fingers of people in Japan, and I am at present collecting others from different nationalities, which I hope may aid students of ethnology in classification. Some few interesting points may here be mentioned by way of introduction.

Some individuals show quite a *symmetrical* development of these furrows. In these cases all the fingers of one hand have a similar arrangement of lines, while the pattern is simply reversed on the other hand. A Gibraltar monkey (*Macacus innus*) examined by me had this arrangement. A slight majority of the few Europeans I have been able to examine here have it also.

An ordinary botanical lens is of great service in bringing out these minor peculiarities. Where the loops occur the innermost lines may simply break off and end abruptly; they may end in self-returning loops, or, again, they may go on without breaks after turning round upon themselves. Some lines also join or branch like junctions in a railway map. All these varieties, however, may be compatible with the general impression of symmetry that the two hands give us when printed from.

In a Japanese man the lines on both thumbs form similar spiral whorls; those of the left fore-finger form a peculiar oval whorl, while those of the right corresponding finger form an open loop having a direction quite opposite to that of the right fore-finger in the previous example. A similar whorl is found on both middle fingers instead of a symmetrically reversed whorl. The right ring-finger again has an oval whorl, but the corresponding left finger shows an open loop.

The lines at the ulno-palmar margin of this particular Japanese are of the parallel sort in both hands, and are quite symmetrical, thus differing from the Englishman's considerably. These instances are not intended to stand for typical patterns of the two peoples, but simply as illustrations of the kind of facts to be observed. My method of observation was at first simply to examine fingers closely, to sketch the general trend of the curves as accurately as possible, recording nationality, sex, colour of eyes and hair, and securing a specimen of the latter. I passed from this to "nature-printing," as ferns are often copied.

A common slate or smooth board of any kind, or a sheet of tin, spread over very thinly and evenly with printer's ink, is all that is required. The parts of which impressions are desired are pressed down steadily and softly, and then are transferred to slightly damp paper. I have succeeded in making very delicate impressions on glass. They are somewhat faint indeed, but would be useful for demonstrations, as details are very well shown, even down to the minute pores. By using different colours of ink useful comparisons could be made of two patterns by superposition. These might be shown by magic lantern. I have had prepared a number of outline hands with blank forms for entering such particulars of each case as may be wanted, and attach a specimen of hair for microscopic examination. Each finger-tip may best be done singly, and people are uncommonly willing to submit to the process. A little *hot* water and soap remove the ink. Benzine is still more effective. The dominancy of heredity through these infinite varieties is sometimes very striking. I have found unique patterns in a parent repeated with marvellous accuracy in his child. Negative results, however, might prove nothing in regard to parentage, a caution which it is important to make.

I am sanguine that the careful study of these patterns may be useful in several ways.

1. We may perhaps be able to extend to other animals the analogies found by me to exist in the monkeys.

2. These analogies may admit of further analysis, and may assist, when better understood, in ethnological classifications.

3. If so, those which are found in ancient pottery may become of immense historical importance.

4. The fingers of mummies, by special preparation, may yield results for comparison. I am very doubtful, however, of this.

5. When bloody finger-marks or impressions on clay, glass, &c., exist, they may lead to the scientific identification of criminals. Already I have had experience in two such cases, and found useful evidence from these marks. In one case greasy finger-marks revealed who had been drinking some rectified spirit. The pattern was unique, and fortunately I had previously obtained a copy of it. They agreed with microscopic fidelity. In another case sooty finger-marks of a person climbing a white wall were of great use as negative evidence. Other cases might occur in medico-legal investigations, as when the hands only of some mutilated victim were found. If previously known they would be much more precise in value than the standard *mole* of the penny novelists. If unknown previously, heredity might enable an expert to determine the relatives with considerable probability in many cases, and with absolute precision in some. Such a case as that of the Claimant even might not be beyond the range of this principle. There might be a recognisable Tichborne type, and there might be an Orton type, to one or other of which experts might relate the case. Absolute identity would prove descent in some circumstances.

I have heard, since coming to these general conclusions by original and patient experiment, that the Chinese criminals from early times have been made to give the impressions of their fingers, just as we make ours yield their photographs. I have not yet, however, succeeded in getting any precise or authenticated facts on that point. That the Egyptians caused their criminals to seal their confessions with their thumb-nails, just as the Japanese do now, a recent discovery proves. This is however quite a different matter, and it is curious to observe that in our country servant-girls used to stamp their sealed letters in the same way. There can be no doubt as to the advantage of having, besides their photographs, a nature-copy of the for-ever-unchangeable finger-furrows of important criminals. It need not surprise us to find that the Chinese have been before us in this as in other matters. I shall be glad to find that it is really so, as it would only serve to confirm the utility of the method, and the facts which may thus have been accumulated would be a rich anthropological mine for patient observers.

HENRY FAULDS

Tsukiji Hospital, Tokio, Japan

[Some very interesting examples of nature-printed finger-tips accompanied this letter.—ED.]

Metamorphic Rocks, Ireland

THERE appears to be confusion as to the times when metamorphic action occurred among the Irish rocks; my experience would point to the following:—

In the Carnsore district, South-East Wexford, there are metamorphic rocks for a long time supposed to be of Lower or Cambro-Silurian age; I however proved that they were upturned, contorted, metamorphosed, and denuded, prior to the overlying fossiliferous Cambro-Silurian rocks being deposited, and for the reasons given in the Geological Survey Memoir it is probable these metamorphic rocks are of Cambrian age.

In the hills north of Pomeroy, Co. Tyrone, there are metamorphic rocks, which were upturned, contorted, metamorphosed, and denuded, prior to the overlying fossiliferous "Pomeroy rocks" having been deposited. The fossils in the latter would point to their being Cambro-Silurians; consequently the metamorphic rocks are older, and for reasons given in a paper read before the Royal Irish Academy I believe they are the equivalents of the "great micalite series," West Galway, or the equivalents of the Arenig group of Wales. Thal is either Upper Cambrian, or *Passage beds* between the Cambro-Silurian and Cambrian.

In Erris, North-West Mayo, there is a tract of excessively metamorphosed rocks, supposed by Griffith to be older than the associated altered Cambro-Silurians, and this opinion is shared in by Mr. McHenry, who more recently examined them.

From the above it is evident that there was a *period of intense metamorphosis prior to the Cambro-Silurian age.*

The Cambrian (Arenig group?) and Cambro-Silurian of Galway and South-West Mayo must, in part, have been altered prior to the deposition of the Upper Silurians on them; while the general metamorphism of the South-East Ireland Cambro-Silurians, *which was quite irrespective of the intrusion of the Leinster granite*, was probably at about the same time. If the Comeragh Mountain rocks are Glengariff grits, *i.e.* Silurians, the age of the metamorphic action is evident, as in Waterford these rocks underlie those of the Comeragh Mountains. In addition to the general metamorphism in the rocks of South-East Ireland, there was also a local and secondary action in connection with the protrusion of certain granitic rocks.

The testimony of the West Galway and South-West Mayo rocks alone, however, would prove *a period of intense metamorphic action at the close of Cambro-Silurian time.*

In South-West Mayo, as proved by Mr. Symes and myself (*Maps and Memoirs of the Geol. Survey*), there is a considerable area of metamorphosed Upper Silurian rocks, which prove *another period of intense metamorphic action subsequent to the dawn of Upper Silurian times.* The secondary metamorphism previously mentioned in South-East Ireland may also be of this age, as the granitic rocks allied with the metamorphic, in both areas, are very similar.

Thus there are records of at least three periods of intense metamorphic action, and probably there were two others subsequently—one in the Triassic and another in the Miocene time—to account respectively for the metamorphic rocks in the neighbourhood of the Mourne granite, Co. Down, and those associated with the granitic rock near Portrush, Co. Antrim.

Formerly, as mentioned by me in the "Geology of Ireland," the period of greatest metamorphism was considered to have been at the close of the Cambro-Silurian time; now, however, more recent research has taught us that metamorphic rocks, formerly supposed to be Cambro-Silurians, are Cambrians; so it seems possible the metamorphic action prior to Cambro-Silurian time may have been greater than that subsequent to it.

Ovoca, Ireland G. H. KINAHAN

The Number of Known Species of Hemiptera-Heteroptera

As Mr. Pascoe, in his very "handy book of reference" for zoological classification, says of the Hemiptera-Heteroptera that "in round numbers there may be about 10,000 species in this sub-order," I am induced to give my census of the group.

On completing, about a year ago, MS. lists of the families which Stål unfortunately did not live to include in his "Enumeratio Hemipterorum," I was tempted to try and ascertain the total number of species that had been described. This I found to be about 7,800 (the actual number arrived at is 7,780). Of these, 7,445 belong to the Geocorisæ or Gymnocerata (mostly terrestrial bugs, but including four families which inhabit the surface of water), and 334 to the Hydrocorisæ or Cryptocerata (almost all aquatic species). Of the Geocorisæ 1,503 are European, 3,248 are natives of the rest of the Old World, and 2,694 are American; of the Hydrocorisæ the corresponding numbers are 95, 120, and 119. The largest family of the Geocorisæ in Europe is the Capsidæ with 500 species, as against 134 and 312 in the rest of the Old World and America respectively. Amongst the Hydrocorisæ the family Corixidæ is most numerous in species, the numbers being: for Europe 72, the rest of the Old World 17, and America 34. But as these two families contain many inconspicuous species, and species having a strong resemblance *inter se*, and as Europe has been (naturally) more thoroughly investigated than the other regions, it is likely that many extra-European species of these families remain yet to be discovered.

Of what may be the actual number of species of Hemiptera-Heteroptera existing it is difficult to form an estimate. It is only of late years that much attention (comparatively) has been directed to the order, and from the number of new species sent home by the few collectors who condescend to collect bugs, it is evident that very great additions to the list will in course of time be made. Even within the last twenty years the list has been more than doubled, as in A. Dohrn's catalogue, published in 1859, only 3,627 are mentioned.

Of the sub-order Homoptera it would be rather difficult to make a census. In Dohrn's catalogue somewhere about 3,000 species are catalogued—a number not very far short of that of the Heteroptera. In the British and European lists the number of Homoptera is about two-thirds that of the Heteroptera.

Perth, October 19 F. BUCHANAN WHITE

On the Classification of Rivers

IT has often occurred to me that a convenient classification of rivers might be obtained by arranging them according to their "water-discharge." Such a classification would not only indicate the relative position of one river to another in a descending scale, but would enable a rough estimate to be borne in the memory of the amount of water any particular river may discharge.

I therefore venture to suggest the following arrangement: and have given below the names of seventeen rivers, the discharges of which I have obtained from various sources, for which I would refer the reader to NATURE, vol. xxii. p. 486.

Discharge of Cubic Feet per second

First Rate. Above 2,000,000.	Second. Above 1,000,000.	Third. Above 500,000.	Fourth. Above 250,000.	Fifth. Above 100,000.
Amazon.	Congo.	Yang'tse. Plate. Mississippi.	Danube. Shat-el-Arab.	Ganges. Indus. Atrato. Nile. Yellow River.

Sixth. Above 50,000.	Seventh. Above 25,000.	Eighth. Above 10,000.	Ninth. Above 5,000.	Tenth. Below 5,000.
Rhone. Rhine. Po.			Pei-ho.	Thames.

Woodlane, Falmouth, October 19 H. B. GUPPY

Yuccas under Cultivation

IN NATURE, vol. xxi. p. 315, in the report of the *Proceedings* of the Linnean Society, it is stated that "the yuccas fruit rarely under cultivation, the large white pendulous flowers being in the wild plant fertilised by a moth of the genus *Pronuba*." The yucca has been introduced and is very abundant in this colony, especially round Noumea. It fruits freely; in fact I rarely see a plant in which many, if not most, of the flowers do not produce seed-pods. In my own garden they seem to be fertilised by the common bee, of which I have a hive, others being in the neighbourhood. If I remember rightly, *Pronuba* is a genus of large moths having yellow underwings. We have a species identical with, or closely resembling, an old Ceylon friend, but it is rare; still it does exist here, and *may* assist in the fertilisa-

tion, though I should say, from the number of flowers fertilised, that other agencies preponderate. E. L. LAYARD

British Consulate, Noumea, New Caledonia, July 31

Intellect in Brutes

I CONFESS I do not see much "intellect" in a snake biting its own tail (cf. NATURE, vol. xxii. p. 40); on the contrary, I consider the creature evinced remarkable stupidity. Perhaps however you will think what I now relate will show that snakes do possess reasoning powers.

Many years ago, while in Ceylon, I lived in a house in "Slave Island," raised on a high platform. The steps up to the door had become loosened, and behind them a colony of frogs had established themselves. One morning I watched a snake (a cobra) creep up, insert his head into a crack, and seize a frog, which he there and then swallowed. But the crack that admitted the thin flat head and neck of the ophidian would not permit of the same being withdrawn when the neck was swollen with the addition of the frog inside it. The snake tugged and struggled, but in vain, and after a series of futile attempts disgorged its prey and withdrew its head. But the sight was too tantalising. Again the head was inserted in the crack and the coveted morsel swallowed, and again the vain struggles to withdraw were renewed. *I saw this repeated several times*, till, gaining wisdom by experience, the snake seized the frog by one leg, withdrew it from its coigne of vantage, and swallowed it outside. E. L. LAYARD

I SEND you the following dog story, the truth of which is vouched for by the young lady who owned the animal. Her pet dog, a black-and-tan-terrier, was well known to the neighbours for his intelligence. He had established a remarkable friendship for a certain kitten, although given to fierce attacks on all others. This kitten was infested with fleas, which, when the dog discovered, he took her by the nape of the neck, in truly parental fashion, and *soused her up and down in a bucket of water*. He would then take her out into the sunshine and carefully pick out the drowned fleas.

A friend of mine, a naturalist, and a very conscientious man, whose word can be implicitly trusted, gives the following, to which he was an eye-witness. His grandfather, then a very old but hale and hearty man, had a splendid Newfoundland. There was a narrow and precipitous road leading from the fields to the house. It was regarded as a very dangerous place. One day when the old gentleman was doing some work about the farm his horse became alarmed and started off with the wagon along this causeway. The chances were that he would dash himself and the empty wagon to pieces. At once the dog seemed to take in the situation, although until that time he had been impassive. He started after the horse at full speed, overtook him, caught the bridle, and by his strength arrested the frightened creature until help could reach him. My friend gives many other stories of this fine dog, and thinks he had a decided sense of humour. I will repeat that both of these tales come to me well authenticated, and I could, by seeking permission, give names and places. W. WHITMAN BAILEY

Broun University, Providence, R. I. (U.S.A.), October 10

Atmospheric Phenomenon

LAST evening (October 21) at 5.45 p.m. I observed four huge radiating arms of faint white light, like the spokes of a gigantic wheel, rising from a centre apparently on the west-south-west horizon, and extending almost to the zenith. I say apparently on the west-south-west horizon, because an intervening house prevented me from seeing the nucleus of the diverging rays. The aspect of the phenomenon was more suggestive of an aurora than anything else I know of, but the beams of light seemed to be quite stationary, and although I fancied their brilliancy increased at one time for a few moments, I cannot be sure. Other fainter rays appeared to me to divide the west-south-west sky with those I have mentioned; but on that point I am also not sure. The sun set at 4.53 p.m., and twilight ended about 6.43 p.m., at which time the appearance I have attempted to describe was no longer visible. The sky was heavily clouded.

I should very much like to know the cause of this (to me) singular exhibition of light. B.

Kentish Town, N.W., October 22

Temperature of the Breath

WITH reference to the high reading, 107°-108°, noticed by Dr. Dudgeon when a thermometer tightly wrapped up in the folds of a silk handkerchief was kept in the mouth for five minutes, might I ask Dr. Dudgeon if he has verified this reading by immersing the thermometer, with a handkerchief tightly rolled round its bulb, in a vessel of water, at say 108°, the temperature of the water being simultaneously taken by a standard thermometer with its bulb uncovered? It seems to me that there is some danger of actually squeezing up the reading of a delicate thermometer when twenty or thirty folds of a silk handkerchief tightly encircle its bulb. F. J. M. P.

October 23

Crossing Rapid Streams

HAVING read some letters lately in your paper on the subject of crossing rapid streams by means of carrying heavy stones, it strikes me that the following may be of interest to your readers. It is an extract from a survey report by Lieut. (now Major) Woodthorpe, R.E., written in 1876, describing the method, which he saw practised by men of the Nàgà tribes, for crossing a deep stream too rapid for their feeble powers of swimming, and about twenty yards wide:—

"Taking large stones in their hands, they waded in up to their necks, and throwing up their legs and lowering their hands, the stones carried them to the bottom, along which they crept on all-fours till they reached the shallows on the other side."

The rough bottom afforded them sufficient hold to withstand the modified current and resist flotation. C.

Mussoorie, September 28

Construction of Telescopes and Microscopes

PERHAPS some of your readers may be able to inform me whether there exists in English or French a work on geometrical optics, in which the author applies himself thoroughly to explain the optical (not the mechanical) construction of telescopes and microscopes. Works like those by Parkinson and Polter stop short exactly where the application of theory to the construction of the best instruments begins. P. C.

September 30

BENJAMIN PEIRCE, F.R.S.

WE regret to have to record the death at Cambridge, Mass., on October 6, of Prof. Peirce of Harvard University, following upon an illness of three months from Bright's disease. Prof. Peirce was the son of a former librarian of the university, Benjamin Peirce, who died in 1831. For the past thirty-five years he has occupied a professorship at Harvard; and as a lecturer, author, thinker, and investigator, has not only ranked amongst the first of a numerous corps of professors, but also among the first of American men of science. Devoting himself originally to mathematics, Prof. Peirce has successively pursued exhaustive studies in all the branches more closely allied to mathematics, and has obtained eminence equally in physics, astronomy, mechanics, and navigation. His numerous investigations in these various departments, while read before various scientific societies, have been published, unfortunately, for the most part in the briefest possible form, and the results of many of his researches are to be found only in the manuals he published on various subjects. As an author Prof. Peirce was highly esteemed upon both sides of the Atlantic, his work on analytical mechanics, which appeared in 1857, being regarded then even in Germany as the best of its kind. His chief works are a "Treatise on Algebra," a "Treatise on Plane and Solid Geometry," "Pure Mathematics," a "Treatise on Sound," "Ocean Lanes for Steamships," "Tables of the Moon," "System of Analytic Mechanics," "Potential Physics," "Linear Associative Algebra," "Analytic Morphology," and "Criterion for the Rejection of Doubtful Observations." As a lecturer Prof. Peirce was highly esteemed in both scientific and popular circles. It is related that in 1843, by a series of popular

lectures on astronomy, he so excited the public interest that the necessary funds were supplied for erecting an observatory at Harvard. A remarkable series of lectures on "Ideality in Science," delivered by him in 1879 before the Lowell Institute in Boston, attracted the general attention of American thinkers, on account of the thoughtful consideration of the vexed question of science and religion.

Much of Prof. Peirce's activity was absorbed by his duties as the head of the American Coast Survey, a position in which he succeeded Prof. Bache. He brought to this work the same degree of zeal and ability which were so brilliantly evidenced by his predecessor, and constantly maintained the well-earned reputation of the Coast Survey among the hydrographic efforts of our day. Prof. Peirce was one of the founders of the American National Academy of Sciences. In 1853 he presided over the American Association for the Advancement of Science. The degree of LL.D. was conferred upon him twice, by the University of North Carolina (1847), and by Harvard University (1867). He was elected an Associate of the Royal Astronomical Society (1849), and a Fellow of the Royal Society of London (1852), and of the Royal Societies of Edinburgh and Göttingen.

Prof. Peirce leaves behind him his wife, a daughter, and three sons. Of the latter one is Professor of Mathematics at Harvard, and another is Professor at Johns Hopkins University.

RECENT CHEMICAL RESEARCH

THE masses of facts accumulated in the text-books on chemistry are already portentous: each month, almost each week, adds to the store.

The difficulty of getting a stable standing-ground from which to survey, in order, if possible, to find the meaning of these facts, increases likewise.

Fortunately from time to time there are found investigators who, turning from the easy toil of adding new compounds to those which are as yet but imperfectly known, concern themselves with the fundamental questions of chemical science.

Why are the properties of bodies so largely modified under certain conditions? This is the all-important question for the chemist. Before this question can be answered for a series of substances the properties of those substances must be accurately known, and the variations in their properties under varying conditions—themselves definitely ascertained—must be determined. Among the properties of substances those which we usually call physical are, as a rule, more susceptible of accurate measurement than those which we call chemical.

But these physical properties must be connected in some way with the chemical structure of the little parts, or molecules, of which we conceive the substances to be built up.

To determine what this connection is in the case of a definite physical property, and for a series of chemical substances, is at present one of the most promising problems which presents itself to the chemical inquirer.

But these physico-chemical problems require for their solution, a practical knowledge both of chemical and physical methods; methods of laboratory work and methods of reasoning on the results obtained. Students of nature trained in both methods are not extremely abundant.

The suggestion made in the preface to Armstrong and Grove's new volume on Organic Chemistry, that each chemical school should regularly prepare special series of pure compounds, and should let it be known that physical observers can procure these compounds in order to determine their physical properties, is well worthy of being acted on by all in whose hands may rest the arrangement of the work of any chemical school.

The older method of regarding chemical physics as consisting of a little chemistry loosely tacked on to a great deal of ordinary physics, is disappearing; and chemists and physicists now recognise that the problems which each attacks are, in very many instances, but different aspects of the same question.

The more thoroughly the chemical worker is trained in the correct use of dynamical principles and dynamical reasoning, the more likely is he to succeed in his search for chemical truth.

Very recently two papers have appeared, the contents of which illustrate the importance of the results obtainable by physico-chemical methods.

Brühl has published in Liebig's *Annalen*—and in a condensed form in the Berlin *Berichte*—the results of his investigations on the connection between physical properties and chemical constitution of carbon compounds; and Thomsen, in the *Journal für practische Chemie* (and also in the *Berichte*) has given the first two instalments of his thermal work bearing on the isomerism of carbon compounds.

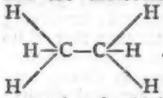
I propose to give a short account of the work of these two chemists: let us begin with Thomsen's.

The "heat of formation" of a compound substance is the difference between the sum of the heats of combustion of the constituent elements of the compound, and the heat of combustion of the compound itself. But this heat is not the true "heat of formation" of the molecule of the compound; it is only the algebraic sum of various heat disturbances. The thermal change which accompanies the formation of a compound molecule from various elementary molecules consists of various parts: (1) heat absorbed in dissociating the molecules of the different elements; (2) in some cases, heat absorbed in liquefying or gasefying the constituent elements; (3) heat evolved in the formation, from the dissociated elementary atoms, of the new compound molecules; and (4) in some cases heat evolved in the liquefaction or solidification of the gaseous compound molecules. If the physical state of the various substances concerned be constant throughout the experiment, (2) and (4) may be neglected; and the heat of formation will be equal to the difference between the heat absorbed in splitting the elementary molecules, and that evolved in the falling together of the atoms so produced, in the new configuration. The value of the first part of this operation will always be constant for the same element or elements; but the value of the second part will depend upon the configuration assumed by the elementary atoms in the new compound molecules.

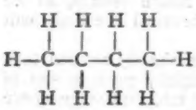
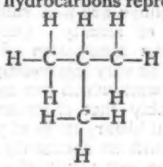
Now the generally accepted chemical theory of isomerism is that it (isomerism) is dependent on varying configuration of the same atoms. Some chemists have urged that isomerism is more probably due to the possession, by the different compounds, of different amounts of energy. But these two theories are really parts of the same theory. Thomsen's method, indeed, may be said to be based on this fundamental identity.

Given the dissociated elementary atoms, they may arrange themselves in various ways, each arrangement will be attended with a definite but different evolution of heat, hence, inasmuch as the heat absorbed in the preliminary elemental dissociation is fixed, the heats of formation of the various isomeric molecules will be different.

But when it is said that isomerism depends on atomic configuration, two things are included in this statement. Let us consider isomerism in a hydrocarbon: the carbon atom combines with four, and not more than four, hydrogen atoms to form a compound molecule. The carbon atom is said to be tetravalent; this is usually graphically expressed by the symbol $=C=$. The maximum number of hydrogen atoms which two carbon atoms can combine with to form a definite molecule will be six,

and the molecule will be graphically represented as  . But we also know of a compound the molecule of which contains two carbon, but only four hydrogen atoms, this is represented as $\frac{H}{H}\!>\!C{=}C\!<\!\frac{H}{H}$, and a third hydrocarbon, C_2H_2, is represented as H—C≡C—H. In the first molecule the carbon atoms are commonly said to be "singly-linked," in the second "doubly-linked," and in the third "trebly-linked." We do not as yet attach any definite physical conception to these phrases; a compound said to contain "singly-linked" carbon atoms is, as a fact, incapable of combining with hydrogen or other monovalent element, whilst a compound said to contain "doubly-linked" carbon atoms can combine with two monovalent atoms for each pair of doubly-linked carbon atoms it is represented as containing; and a compound said to contain "trebly-linked" carbon atoms is capable of combining with four monovalent atoms for each pair of trebly-linked carbon atoms in the graphic formula thereof.

These are instances of isomerism said to be due to differences in the linking of the atoms of the isomeric molecules. But according to the generally accepted theory isomerism may arise among hydrocarbons in which all the carbon atoms are singly-linked; such isomerism is due to different relative arrangements of parts of the molecule. We may suppose all the carbon atoms arranged in a chain, or we may suppose ramifications of these atoms; thus the hydrocarbons represented as

 and  would be isomeric.

Thomsen deals only with isomerism due to differences in the linking of atoms. If from a certain number of dissociated carbon and hydrogen atoms a compound be produced containing only "singly-linked" carbon atoms, that compound is not capable of taking up any more hydrogen; but if a compound be produced containing "doubly-linked" carbon atoms, that compound is capable of taking up more hydrogen. But in the act of combining with more hydrogen, heat will be evolved; hence the heat of formation of the first compound is greater than that of the second. The heat of formation of an isomeric compound containing "trebly-linked" carbon atoms would be less than that of either of the preceding.

Thomsen, from the results of his own and other experiments, has calculated the heat of formation, from amorphous carbon, of a pair of singly-linked, a pair of doubly-linked, a pair of trebly-linked, and a pair of quadruply-linked carbon atoms. From these values he has calculated the heats of formation of isomers containing singly, doubly, or trebly-linked carbon atoms. The calculations involve certain assumptions, but the applications of his results to actual hydrocarbons show very close agreement between the calculated and the actually determined "heats of formation."

Thomsen furnishes us with a thermal value for the formation of each of the three possible linkings of the group C_2 in the molecule of compounds. The value of this result to the chemist is great; a determination of the heat of combustion of a hydrocarbon may now yield him much information as to the structure of the molecule of that hydrocarbon.

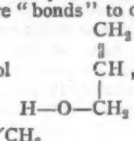
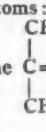
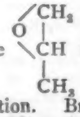
Thomsen's results also strengthen the commonly-accepted theory of isomerism, and they point towards a dynamical explanation of this theory and to the possibility of attaching a definite physical idea to the phrases "singly" or "doubly-linked" atoms.

As Thomsen has succeeded in tracing a quantitative connection between the heats of formation of certain molecules containing carbon and the linking of the carbon atoms in these molecules, so Brühl has shown that the linking of carbon atoms exerts a definite, measureable influence on the *molecular refractions* of compounds of this element.

Landolt showed many years ago, that in many compounds, the atoms of each elementary substance, possessed a definite specific refractive capacity independently of the way in which the atoms might be grouped.

Molecular refraction is defined as $\left(\frac{\mu - 1}{d}\right)$. M, where μ = refractive index, d = density of substance, and M = molecular weight.

The difference between the molecular refraction of a compound containing carbon, hydrogen, and oxygen, and that of a compound containing the same number of carbon and hydrogen atoms, but free from oxygen, gave the atomic refraction of oxygen. Numbers were thus found expressing the atomic refraction of carbon, hydrogen, oxygen, and a few other elements. Gladstone and Dale showed, however, that the observed molecular refractions of many carbon compounds, especially the compounds existing in essential oils, were greater than the refractions calculated from Landolt's numbers: it seemed that the grouping of atoms did exert, in certain cases, an influence on the refractive power of molecules.

Brühl finds that certain groups of isomeric carbon compounds possess but one molecular refraction; in these groups the refractive power of the molecules is independent of the grouping of the atoms; in other isomeric groups, however, the molecular refraction varies. The members of the latter groups of isomers are always represented in structural formulæ as containing "doubly-linked" carbon atoms. Now if the molecular refraction be conditioned by the linking, but not by the grouping, of the atoms in the molecule, it follows that the atomic refraction of each monovalent element must be a constant number, inasmuch as there is but one way of linking a monovalent atom to other atoms. Such isomers as ethylene chloride, $ClH_2C—CH_2Cl$, and ethylidene chloride, $Cl_2HC—CH_3$, should possess the same molecular refraction. But the atomic refraction of any polyvalent atom, *e.g.* oxygen, must vary according as the atom is linked by one, two, or more "bonds" to other atoms: such isomers as allylic alcohol , acetone , and propylene oxide  should possess each a distinct molecular refraction. Brühl's actual results confirm these deductions. There is then a definite value for the atomic refraction of the carbon, or oxygen, atom according as that atom is "singly-linked" or "doubly-linked" to other atoms: in other words, the molecular refraction of a compound containing the group $\!>\!C—C\!<\!$ is different from that of the isomer containing the group $>\!C{=}C\!<$; and the molecular refraction of a compound containing the group $>\!C{=}O$ is different from that of the isomer

containing the group $\rangle C—O—$. Brühl obtains a definite numerical value for the refractive power of each of these groups.

Now although the molecular refraction of isomers with similarly linked, but differently grouped, carbon or oxygen atoms is constant, the refractive indices and the densities of these isomers are not the same. There is, therefore, a definite connection between the densities and refractive indices of carbon compounds, and the grouping, as distinguished from the linking, of the atoms in these compounds. The densities and refractive indices of the isomers, ethylene chloride and ethylidene chloride (see *ante*) are not the same. Brühl has not determined any exact numerical value for the refractive power of this or that grouping of carbon or other atoms; generally, however, he has shown that the more ramifications there are in the structural formula of a carbon compound, the smaller is the density and the smaller the refractive index of that compound. Thus the density of butylic iodide, $CH_3—CH_2—CH_2—CH_2I$, is 1·6166, and the refractive index (μ) is 1·49601; but the density of the isomeric isobutylic iodide, $\begin{array}{c} CH_3—CH—CH_3 \\ | \\ CH_2I \end{array}$, is 1·6056, and the refractive index is 1·49192.

Generally, then, it would appear that when rays of light pass through a series of isomeric carbon compounds, the isomerism of which is traceable only to differences in the grouping of the constituent atoms, then that ray which passes through the densest compound is more bent from its original course than any of the other rays; but that when isomerism is due to differences in the linking of the atoms, then the amount to which the rays are bent is dependent not only on the density, but also on the molecular "structure" of the compounds.

Brühl considers also the connection existing between the boiling points, and other physical constants, of isomeric carbon compounds containing only singly-linked polyvalent atoms, *i.e.* compounds the isomerism of which is due only to variations in the grouping of the atoms, and the structural formulæ of these compounds. His results establish a considerable probability in favour of the rule, that in such isomeric groups, those compounds which have the smallest molecular volumes, have also the highest boiling points, greatest specific gravities and refractive indices (*not* greatest *molecular refraction*), and longest time of flow through capillary tubes; and very probably these compounds have also the smallest amount of ramification in their molecular structure.

Brühl thus put into the hand of the chemist another means whereby he may readily learn much concerning the inner structure of the substances which he examines. Brühl's results, as also those of Thomsen, exhibit a close connection between physical properties of compounds and the valency, or specific saturation power, of the elementary atoms which build up these compounds.

As the theories of modern chemistry are so largely based on the idea of valency, the results of Brühl and Thomsen are most welcome, as at once tending to confirm the general soundness of the methods of the Newer Chemistry, and exhibiting at least two measurable physical phenomena as closely connected with the exercise of valency.

The results of both observers emphasise the difference which chemists have long recognised between two kinds of isomerism: that due to "grouping," and that due to "linking" of atoms. Is it not at least possible, in view of these results, that a greater part of the chemical energy of molecules containing doubly (or trebly) linked polyvalent atoms is kinetic, than is the case in isomeric molecules, the atoms of which are all singly-linked? if indeed the chemical energy of the latter molecules be not wholly potential. Double-linking might then mean greater kinetic energy; and the entropy of a molecule containing only singly-linked atoms would be greater than that of its isomer, some of the atoms in which were doubly-linked.

The consideration of valency of atoms is closely connected with the more general subject of chemical affinity; and the work of Thomsen and Brühl suggests many questions connected with affinity which press for answers.

A short account was given in this journal (vol. xx. p. 530) of the work of Guldberg and Waage, and of Ostwald, on chemical affinity. The latter naturalist has recently extended his methods of observation: in his earlier papers he used physical methods, determining the changes in the specific volumes, and also in the refractive indices, of solutions of acids and bases when these acted chemically on each other, and hence calculating the amount of chemical action. Ostwald now employs a more purely chemical method; he allows acids of known strength to react on a solid salt in excess, and determines the amount of action at definite intervals. His results, so far as they have extended,[1] strikingly confirm the numbers which he before obtained for the relative affinities of the commoner acids.

The application of the theory of Guldberg and Waage to reactions between a solid and a liquid, the former being in excess, requires that a definite and stable condition of equilibrium should be reached at the expiry of not too great a time. Doubt was thrown on Ostwald's results because it was said that such equilibrium had not been attained. In his latest paper Ostwald has carefully examined this point, and has shown that the required equilibrium is attained, and maintained, and that therefore such reactions are well suited for the study of general problems of affinity. Ostwald's future results, as he extends the application of the chemical method, will doubtless be very interesting.

All the work which has been here shortly noticed tells unmistakably that chemistry is rapidly passing out of the natural history stage of progress into that stage where her facts will be accurately grouped under general laws, which laws will admit of quantitative statement, and of quantitative deductions being made from them.

The recent work in chemistry also illustrates the need of a wide training in the methods of various sciences for the investigator of this branch of natural phenomena. One man begins with a purely chemical investigation, another with one which appears wholly physical; before long they find that their paths meet, and that the problem which each had attacked without thought of the other, can be solved, and even then solved but partially, only by the united effort of both.

M. M. PATTISON MUIR

JAPAN[2]

I.

MR. MURRAY is to be congratulated on being able to bring out simultaneously two such excellent books on a country which for some years has probably attracted more interest than any other country in the world. Although they both treat of the same subject, they differ much in their method of treatment. Indeed the one may be said to be complementary of the other; and any one who reads them both with care will be able to form a very complete idea of the present condition of an unusually interesting country and people. Sir Edward Reed went out practically as the guest of the Japanese Government, and had ample opportunities of seeing the

[1] His papers are in the *Journal für practische Chemie* of the last and present year.

[2] "Japan: its History, Traditions, and Religions, with the Narrative of a Visit in 1879." By Sir Edward J. Reed, K.C.B., F.R.S., M.P. Two vols. With Map and Illustrations. (London: John Murray, 1880.) "Unbeaten Tracks in Japan." By Isabella L. Bird. Two vols. With Map and Illustrations. (Same Publisher.)

official side of the life of the country, of gaining a knowledge of what is being done to graft the results of Western civilisation on a civilisation centuries older, and which has been developed on totally different lines. From first to last he was in the hands of the leading Government officials of the country, who spared no pains to make his visit as pleasant as it could possibly be. During the whole of his three months' visit to the country, from the beginning of January, 1879, he had seldom an hour to himself, and what time he could subtract from his sleep was given to the writing up of his notes on his day's work, for work it must have been, harder than even an obstruction night in Parliament. From the young Mikâdo down to the most subordinate provincial official, every one was anxious to convince the great English engineer that the enthusiasm with which they received him was genuine, and that they would only be too glad to let him inspect every detail of the great work they were endeavouring to carry out for their country. From beginning to end his visit to the country was a triumphal progress, and, as might have been expected, Sir Edward Reed left the country with a high opinion of its Government, and deeply impressed with the genuineness and thoroughness of its progress. Miss Bird, on the other hand, went to Japan, as she went to the Sandwich Islands and the Rocky Mountains, solely in pursuit of health, which she sought and found by travelling alone in those parts of the country rarely if ever frequented by foreigners, living in common inns and humble houses, and finishing up with a sojourn among that curious people known as the Ainos, the probable aborigines of Japan. She of course had every protection which the influence of Sir Harry Parkes, our representative, could procure her, and her passport was powerful enough to secure a courteous reception wherever she went; indeed she found travelling safer in Japan than it is in some European countries. To some extent it may be said that Sir Edward Reed was shown the outside and the brightest side of Japanese life, while Miss Bird plodded her way through the unfrequented heart of the country, and saw much of the light and shade in the everyday life of the common people. The two travellers had this in common, that no obstacle was put in the way of their seeing all that they desired to see, leaving one with the conviction that the Japanese Government has really nothing to conceal, and that their enthusiasm for progress is, for the present at least, genuine. Thus the two works, as we have said, afford a fairly complete picture of all sides of Japanese life.

Sir Edward Reed's headquarters were of course at Tokio, where he was courteously received by the young Emperor, who impressed him as a man thoroughly anxious to do the best he can for his people, but old and careworn beyond his years from the many trials he has had to undergo since his accession. Here he met with most of the ministers and other public officials, and he has a good word to say about every one of them. All the public sights were of course seen, and especially the great temples, both Shinto and Buddhist. Indeed a great part of the narrative is occupied with accounts of the numerous temples visited by Sir Edward, their architecture, ornaments, relics, and history, and the legends connected with them; and they seem to be all so much alike that we think some of the space thus occupied might have been devoted to other details of his interesting journey. After a month's stay in Tokio, Sir Edward and his son, who accompanied him, and a few of whose interesting notes are embodied in his father's narrative, were taken in a lighthouse steamer round the south coast of the main island through the Inland Sea to the outside of Shimonoséki Strait. The number of excellent lighthouses, constructed on the very latest principles, is remarkable in a country whose adoption of Western institutions is scarcely ten years old. Various points on the coast were touched at, and the vessel finally left at Osaka. From this point the journey into the interior of the main island and back to Yokohama was performed in those curious man-cabs known as "jinriki-shas," which were only introduced seven years ago, but which look as long-established as cabs in London, up to Kioto, the old capital of the country, down to the sacred city of Nara, and back by the ancient Shinto shrines of Isé, at the south entrance to Owari Bay. During this busy journey the time not devoted to inspecting Shinto and Buddhist temples was spent in visiting public works of various kinds, manufactories, schools of all grades, dining, mostly in Japanese fashion, and being amused by dances and other spectacles of a strictly indigenous kind. How much the great bulk of the people have yet to learn is evident from the fact that in many parts of their route through the most frequented part of the country the people would crowd to the doors and run from their work in the fields to get a look at the "Chinese" riding in their jinriki-shas.

It would be impossible to give the reader any idea of one-tenth of the things which Sir Edward Reed saw and which he tells about. As an engineer he was naturally much interested in the public works and manufactures of the country, and the magnitude of some of the Government factories, and the perfection which they have already reached, impressed and delighted him. Even the engineering feats of Old Japan astonished him sometimes, as in the case of the great blocks of stone in the castle of Osaka, the beauty and grandeur of which he says it would be impossible to exaggerate. "The whole or most of the walls are notable for these very large blocks of granite, which vie with the largest of those built into the great pyramid of Cheops, near Cairo, in Egypt; but as the main entrance to the castle proper is approached, one sees block after block of the most astonishing proportions, until at and opposite to the entrance itself are single stones of such immense size that one is almost driven to doubt whether his senses are not deceiving him. It is so difficult to understand how such huge masses can have been quarried, transported, raised to such a height, and there worked into walls. I could not conveniently measure the largest stones, but I feel sure that some of them must be over twenty feet in height, nearly twice that in length, and several feet thick, and must weigh three hundred to four hundred tons."

Into their paper-manufacture the Japanese have introduced the best modern machinery, and paper has for centuries played an important part in the everyday life of the Japanese. Partitions, table-cloths, napkins, curtains, carriage-covers, and innumerable other things are made of this material, and Sir Edward thinks it would be a good thing to introduce some of the articles thus made into our own country. He paid much attention to the native art of the country, of which it is evident we have the most erroneous ideas. The ordinary reproductions of Japanese pictures which we see here, are wretched caricatures, and in this as in many other points we have much to learn before we have any adequate idea of the real nature of Japanese civilisation. They have ever so many schools of art going back for centuries, and many of their pictures are well worth studying, and capable of affording genuine pleasure. Their method of producing their famous lacquer-work, and their various contrivances for casting, interested him greatly, and he gives much curious information on these and similar matters.

Some idea of the multifarious industries of the country and of the zeal of the Government in encouraging them may be gathered from Sir Edward's account of the industries of Kioto. "Under the city government of Kioto there is an industrial department, the Kuwangiyoba, which was established in 1870 specially for the promotion of the industrial arts, and which has the following branches:—1. An experimental gardening department (Saibaishi

Kenjo), commenced in 1872, for the cultivation of foreign and Japanese fruits and vegetables; 2. A shoe-manufactory (Seikuwajo), begun at the same time, for extending the manufacture of boots and shoes of European style; 3. A weaving-factory (Shokkoba), begun in 1873, where silks and other fabrics are woven, principally in foreign looms: this branch sent three workmen to Europe to learn the art of foreign weaving; 4. A physical and chemical branch (Semikiyoku), which has a sub-branch at Miyadju, in Tango, eighty miles distant, and which, with the assistance of two foreign workmen, is promoting and teaching the manufacture of chemicals, soap, effervescing and lemon drinks, *cloisonné* ware, porcelain, &c.; adjoining it is the Senkojo, for teaching dyeing on foreign methods; 5. The female industrial school, Jokoba, already mentioned; 6. The Bokujo, or more properly Bokuchikujo, which is an experimental farm, established in 1871 with the object of improving the breeding of cattle and of teaching agriculture, the foreign cattle and sheep being chiefly purchased in America, and the milk produced being sold in the city; a branch farm exists at Komo in Tamba, about sixteen miles from Kioto; 7. A department (Yosanba) for promoting the multiplication of silkworms; 8. A pauper industrial department (Jusansho), established in 1869, with a branch at Dosembo, in the south-eastern part of Kioto County, where agriculture and the manufacture of earthenware are the principal employments of the pauper colony; 9. A street-sweeping department (Kuwakaisho), where compost is prepared on the French method; 10. A paper-manufactory, established in 1875. There exist also separate branches for making and teaching how to prepare leather, beer, and mineral waters. A museum is in course of formation."

Of course the educational establishments of the country interested Sir Edward greatly. We have heard much of the admirable university of Tokio and its famous engineer-

FIG. 1.—Mount Fuji.

ing school. But all over the country, at least so much of it as Sir Edward Reed visited, Government is evidently doing what it can to give facilities for education of the best kind. Schools of all grades and for all classes and both sexes are being everywhere established, and some of those Sir Edward visited seemed to be admirably organised, though some of the subjects taught, especially to girls, are amusing. We all know what a hold science has taken upon the Japanese ever since they opened their country to European and American influence. They have been shrewd enough to see that through the encouragement of science lies the surest road to national progress, and the Government has spared no pains nor expense to place education in science in the first rank; and this feature is seen throughout all their schools. The present purpose of the Government is evidently to make education universal all over the country, and to bring it up to a standard equal to that of the foremost countries in Europe. Every soldier Sir Edward Reed noticed in the barracks at Osaka had a little library of books all to himself, and this is a relic of the old days of Japan, when the *samurai* class were at once the soldiers and scholars of the country. Sir Edward is sanguine enough to hope that the time may come in this country when soldiers will occupy a comparatively high position in the social scale, "and when the army will attract to it the surplus members of the civil community of all grades that are respectable and well instructed." Sir Edward was, moreover, struck with the size of the men in various parts of the country, as contrasted with the little fellows that are sent over here to be educated, and with the common idea entertained in Europe of the stature of Japanese. Indeed Sir Edward's testimony on this point is so novel and so different from that which has been generally accepted, that we should like to see some attention given to the subject by those in a position to throw light upon it. Sir Edward met at Kioto Mr. Akamatz, a highly-educated Buddhist priest, who had been to Europe to study and report on the

religions of the West, and who spoke English well. "It may be interesting," Sir Edward says, "to some of my readers to learn that this excellent priest, possessing a knowledge of England and the English, and also the chief priest who was our host on this occasion, find embraced in their section of the Buddhist faith all that they consider good and true in the Christian religion, and are not without hope of seeing England adopt this view, and with it the tenets and practice of their faith, which they consider most excellent. It will be gratifying, doubtless, to the many good people at home who look upon Buddhists as eligible for conversion to their particular views of the Christian religion (whatever they may happen to be in each case), to find their own generous and beneficent intentions so entirely reciprocated."

Over Sir Edward, as over others who have been to Japan, the quiescent (not necessarily extinct) volcano, Fuji-yama, seems to have exercised an influence akin to fascination. He was never tired of looking at the snow-covered cone, rising nearly 13,000 feet above the sea in solitary grandeur, and like no other mountain in the world. For hundreds of miles around it is the prominent feature in the landscape, and the first object that meets the traveller's sight coming from south or east. "But the best evidence of the sacred character of Fuji is to be found, I think, in the fact that every person who speaks or writes about it seems naturally to rise more or less into a reverent state of feeling as he does so. It has a real, a strong, and a solemnising influence on all who behold it. Even when it is viewed from beyond other mountains, its sovereign character is very striking; and when it is seen springing with one tremendous and sublime flight from sea to sky, it is of more sovereign character still."

But the record of what Sir Edward Reed saw while he was in Japan forms a comparatively small part of the two

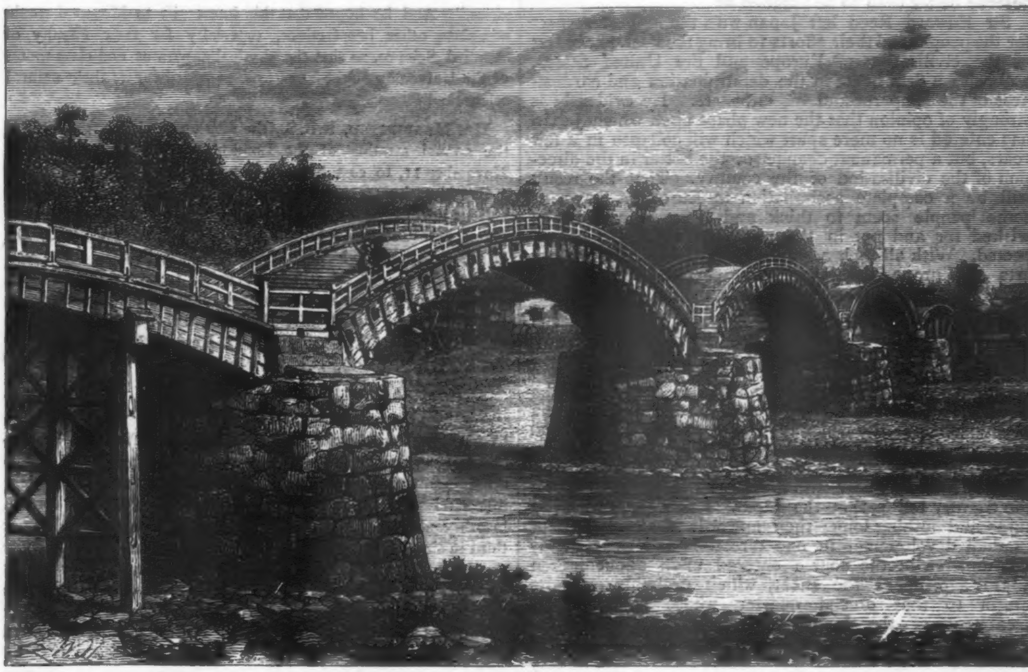


FIG. 2.—Curious Japanese Bridge.

volumes he has written. His interest in the country and its people is so great that he has put himself to considerable trouble to master their history, their religions, their political and social systems, their art and manufactures, in short everything that could enable him to understand a civilisation so real, but so entirely different from anything in Europe. The results of all this study, with the conclusions he has come to both from this and from his visit to the country, occupy a considerable part of the work. That a man of the scientific eminence and political experience of Sir Edward Reed should take so much interest in Japan seems to us a proof that it really deserves the attention of all thoughtful men; and whatever conclusions such an observer may come to ought to have considerable weight with those who are not quite sure what to think of the strange social and political phenomenon that has been taking place for upwards of ten years in the farthest East. Unless, however, the subject is approached in the spirit with which Sir Edward Reed has taken it up, a spirit of thorough seriousness, with an adequate idea of the worthiness of the subject for earnest inquiry, it had better be left alone. A little learning here is a dangerous thing, and has led some triflers to find only amusement in Japanese history and Japanese ways, as if this were merely a toy civilisation, and not a complicated system which has been the development of ages. Sir Edward traces, in his first volume, the history of the Japanese from the earliest "God-period" down to the present time; discusses their two great religions, the native Shintoism and imported Buddhism, their political and social system, their foreign relations, the recent reforms, and the existing government. In the second volume, besides the narrative of his journey, he has interesting chapters on art and on the proverbs and phrases of the people; and both in the second volume and in the introduction he has elaborate

discussions on the ethnology of the Japanese, their language and literature. Sir Edward does not profess to know all these subjects at first hand, but has, with perhaps only one exception, chosen for his guidance the most trustworthy authorities attainable. Sir Edward gives several examples of what the Japanese language is capable of in the way of poetry; we have space for only one specimen:—

"Types of our children are the tiny grasses,
Tender and fragile in the ample moorland:
We know not to what fragrance their infant sprouts may blossom,
Nor wist to what sweetness their unborn fruits may ripen,
But hoping ever wait till autumn tells their story.
Oh! cherished children, may ye never perish,
Flowerless, fruitless, in the early springtime,
Nor like this petal trampled by the wayside,
Fall in the fuller promise of your prime."

A people that are capable of thinking and writing thus deserve better than to be laughed at.

Sir Edward Reed left Japan with the highest respect for the people and their efforts to bring themselves abreast of the civilisation of Europe and the United States, and with a firm belief in the determination and earnestness of the Emperor and his ministers. He evidently is strongly of opinion that the new phase upon which Japan has entered is no mere spurt which will collapse in a few years, but a permanent change for the better in the direction of the civilisation of the country. That the result will be a complete assimilation to European ways, as some people seem to think and hope, is not to be wished for and not in the nature of things to be expected. With all their admiration for the science and the arts of Europe, the Japanese respect themselves sufficiently to see that there is much in their old civilisation that may well be retained. Indeed the problem is one of the meeting of two forces. A new force from an entirely different direction has struck in upon the course of the old civilisation, with the result of a permanent change of direction; but that change cannot be entirely in the direction of the new force. Nor will the final result be a lapse back into the old ways; even in the brief period since the country was opened to European influence the change has been so wide and deep that any such lapse is inconceivable. Those who are in the habit of decrying the country tell us that the Japanese are everything by turns and nothing long; their upwards of 2,000 years of gradual development in one direction, and their steady continuance in the course entered upon about fifteen years ago, belies the sneer, which probably owes its origin to that official quarter whose contemptuous treatment of the Japanese Government Sir Edward Reed so strongly laments. We earnestly hope that the Japanese will go on during the next fifteen years as they have done in the past, and by that time the current in the new channel will be so broad and powerful that it will require a force of equal power to seriously change its direction, and we do not know where that is to come from. The problem in national development being worked out by the Japanese is of the highest possible interest, and what is its real nature cannot be better learned than from the two valuable volumes which so busy a man as Sir Edward Reed has found time to put together.[1]

NOTES

THE foundation-stone of the new museum of McGill College, Montreal, to which we referred some time ago, was laid on September 21 by the Marquis of Lorne. Principal Dawson in thanking Mr. Redpath, the donor, for his generous gift, stated that the museum would be not merely a place for the exhibition of specimens, but a teaching instrument and a laboratory of original research; a great natural science department of the University, in which the classes in geology and biology would receive their instruction, original workers would be trained in all departments of natural science, and from which would go forth the men—and, he trusted, the women also—best fitted to bring to light the hidden treasures of the Dominion, and to avert by the aid of science the injuries with which any of its industries might be threatened. Dr. Dawson referred to other noble examples of private local or national liberality on the American continent, besides those of which Montreal can boast—to "the great National Museum at Washington, which is intended to rival, and if possible surpass, the British Museum; the Central Park Museum of New York, on which that great city has lavished vast sums of money; the Zoological Museum of Harvard, whose revenues would suffice to support some entire universities in this country; or the foundations of Mr. Peabody, which have established great museums in several American cities." And he hoped that this latest gift to Montreal would stimulate other benefactions, especially for their Faculty of Applied Science, so that the physical apparatus and class-rooms of the University might be as well provided for as their natural science collections.

[1] For the illustrations in this article we are indebted to the courtesy of Mr. Murray.

MR. MERRIFIELD, F.R.S., the retiring president, proposes at the annual meeting of the London Mathematical Society on November 11, to cast his valedictory address into the form of "Considerations respecting the Translation of Series of Observations into Continuous Formulæ." The following is the proposed new Council:—Mr. S. Roberts, F.R.S., president; Dr. Hirst, F.R.S., and Mr. J. W. L. Glaisher, F.R.S., vice-presidents; Mr. C. W. Merrifield, F.R.S., treasurer; Messrs. M. Jenkins and R. Tucker, honorary secretaries; other members, Prof. Cayley, F.R.S., Mr. H. Hart, Prof. Henrici, F.R.S., Dr. Hopkinson, F.R.S., Mr. A. B. Kempe, Mr. R. F. Scott, Prof. H. J. S. Smith, F.R.S., Messrs. Lloyd Tanner, H. M. Taylor, and J. J. Walker.

WE take the following from the New York "Monthly Index to Current Periodical Literature," &c.:—"The new Warner Observatory which is being erected at Rochester, N.Y., is attracting much attention in social and literary as well as scientific circles. The new telescope will be twenty-two feet in length, and its lens sixteen inches in diameter, thus making it third in size of any instrument heretofore manufactured, while the dome of the Observatory is to have some new appliances for specially observing certain portions of the heavens. It is to be the finest private observatory in the world, and has been heavily endowed by Mr. H. H. Warner. Prof. Swift has laboured under numerous disadvantages in the past, and the new comet which he recently found was in spite of many obstacles; but as the new institution is to be specially devoted to discoveries, there are good reasons to expect very many scientific revelations in the near future from the Warner Observatory at Rochester."

THE *Times* has shown considerable pluck in having erected at its office one of Mr. Jordan's glycerine barometers, described in NATURE, vol. xxi. p. 377. In the issue of the 25th inst. and following days are published the readings of this gigantic barometer at intervals of two hours from 2 p.m. to 2 a.m. This will be continued regularly, a second edition of the paper giving the two-hourly readings from midnight to noon. These daily records with a barometer on such an enormous scale will be of the greatest value. The *Times* rightly states that it seems unquestionable that an instrument of this kind is admirably suited for practical use at meteorological stations, at seaports, in collieries, and in all other situations where it is of importance for the unpractised eye to notice frequently and easily the changes taking place in atmospheric pressure.

THE results of the observations made from the two balloons sent up from the Crystal Palace on Thursday last have not yet been discussed. But it may be stated that the direction of the wind was remarkably steady, as during the run the two balloons were constantly kept in view of each other in spite of the want of light and transparency of air. This result is all the more to be noted that the variations in the altitude of the two balloons were frequent and considerable, 0 to 5000 feet. The variation of temperature did not amount to more than 5° C. between the maximum of the readings and their minimum. A peculiar current was observed just on arriving on the coast, which is usual under such circumstances. The composition of the clouds was very complex. First, a layer of transparent fog covered almost the whole of the land and gave a watery appearance to it; second, cumuli described as analogous to pulled bread were floating at a height of 1000 metres and descended gradually as the sun was nearing to the horizon; and lastly, a large number of parallel strati stretching south-westerly in the direction of the sun, and seemingly diverging from it. The velocity of the wind was about half a mile per minute, and pretty well determined by observers located in one of the two towers of the Crystal Palace. As to the prognostication of the route, it was nicely done by Mr. Coxwell, who told M. de Fonvielle that he should land between Portsmouth and Winchester. A question arose between M. de Fonvielle and Commander Cheyne about the bearing, the latter's compass having been reversed by an optical illusion, but the azimuth was given with great accuracy, and the uncertainty between the two would not have lasted for a minute if the possibility of the error could have been ascertained. The swinging of the balloon round its axis was sufficient to prevent the use of a new compass designed on purpose for aëronauts.

IT has been represented to us that in our notice of Prof. Owen's work the statement that "he was lecturer on palæontology at the School of Mines in Jermyn Street in 1856" may lead to a misapprehension. We have therefore to state that although Prof. Owen delivered a course of lectures in the theatre of the School of Mines in the year in question, he held no appointment in that institution.

MR. GRAHAM BELL has been honoured in the scientific, as well as other circles of Paris during the past week. He exhibited his photophone at the establishment of M. Antoine Breguet and elsewhere, and was the object of much curiosity wherever he went as "l'homme qui fait parler la lumière."

AT the opening meeting of the Geologists' Association on November 5, the president, Prof. Rupert Jones, will read a paper on the origin and progress of that society.

THE next number of the Victoria Philosophical Institute's *Journal* is announced to contain papers by Prof. Stokes, F.R.S., Prof. Hughes of Cambridge, Prof. Nicholson, M.D., F.R.S.E., of St. Andrew's, and Dr. Hormuzd Rassam, with maps and details of his discoveries.

MR. FLETCHER of Warrington has sent us a specimen of a new gas heating burner which seems well adapted for many purposes and trades which are as yet unsupplied with satisfactory heating apparatus. It seems to us to have all the advantages claimed for it by Mr. Fletcher. It has from three to four times the power of any burner similar in appearance; the flame is *solid*, intensely hot, and perfectly free from smell; it gives a duty higher than the calculated theoretical maximum for the gas consumed, and it cannot be damaged by the dirtiest work. In case the perforated copper dome gets choked with dirt, it can when the burner is warm be lifted off and washed or brushed clean. Any liquid spilt so as to get inside the burner flows out by the side tube without the possibility of damaging the burner. The body of the burner is cast all in one piece, without a joint, thus doing away with one great fault, causing liability to leakage in most of the burners at present in use. Altogether this burner seems to be one of the greatest advances yet made in the practice of heating by gas. Mr. Fletcher has also sent us a useful practical paper on Heating (including cooking) by Gas, read the other day before the Philosophical Society of Glasgow.

PART iii. is to hand of the magnificent "Bedfordshire Pomona," the illustrations of the apples and pears in which continue to be as numerous and life-like as ever, so much so as to make one's mouth water. The papers in this part are on "The Crab, its Characteristics and Associations," by Mr. Edwin Lees, F.L.S.; "The Orchard, its Products: Cider and Perry," by the Rev. C. H. Bulmer; the latter a paper of considerable length, minute detail, and great practical value. Mr. David Bogue is the London publisher.

AMONG the lectures to be given this winter at the Museum and Library, Queen's Road, Bristol, are the following:—November 22, Prof. S. P. Thompson, B.A., D.Sc., "The Rainbow," illustrated with experiments by the electric light; January 17, 1881, Prof. Rolleston, M.A., M.D., F.R.S., F.L.S., Linacre Professor of Anatomy and Physiology, Oxford, "The Early Races of the British Isles"; January 31, Sir John Lubbock, Bart., M.P., F.R.S., F.L.S., "Fruit and Seeds"; February 14, Rev. J. M. Wilson, M.A., F.R.A.S., Head Master of Clifton College, "Double and Multiple Stars"; February 28, Dr. W. H. Stone, F.R.C.S., Lecturer on Physics at St. Thomas's Hospital, "The Measurement and Determination of Musical Pitch," illustrated with experiments; March 14, Prof. W. J. Sollas, M.A., F.R.S.E., F.G.S., Curator of the Bristol Museum, "Coal and the Bristol Coalfields."

WE have received the Catalogue of the General Lending Department of the Newcastle-on-Tyne Public Library, a very thick volume, with a much thinner one containing a list of the books of the Juvenile Lending Department. We may notice them more at length in a future number.

WE have received a very favourable Sixth Annual Report from the West London Scientific Association and Field Club, which commenced its new session on the second Tuesday of this month.

THE *Reports* of the Dunedin (N.Z.) Naturalists' Field Club for 1878-80 are, we regret to see, desponding. It finds some difficulty in keeping up the interest of its members, rather a strange thing in the land of the New Zealand Institute. The Report contains catalogues of the indigenous and introduced flowering plants occurring in the Dunedin district.

ON September 23 Rangoon was visited by three distinct shocks of earthquake; all parts of the province had previously been visited by shocks. A shock of earthquake lasting two seconds was felt at Cordova on the 21st inst., accompanied by a loud subterraneous rumbling. A slight shock, lasting six seconds, was also felt at Madrid on the same day. The shock was stronger in the centre of the city than in the outskirts, and shocks occurred in several towns of the province of Zamora, but no damage has been done. On the same date a shock, the after effects of which were felt in almost every part of the country, occurred both at Lisbon and Coimbra, without however doing any damage.

IT is stated that at the National Exhibition to be opened at Milan next year there will be a captive balloon, on the model of the one which was so successful in Paris in 1878. It will measure not less than 180 feet in circumference, 84 feet in height, and contain 15,000 cubic feet of gas. To it will be

attached a safe and solid car, capable of containing seats for at least eight persons. A steam-engine is to regulate the ascent and descent, and it will rise to a height of about 900 feet, affording a splendid view of Milan and the plains of Lombardy. The balloon will be constructed at Milan, M. Henri Beudet, the well-known and experienced aëronaut, having been sent for to direct the work.

THE coal-beds on the Souris River, Manitoba, have proved very rich, and are to be developed during the winter.

THE Japan papers call attention to the almost limitless mineral wealth lying dormant in the country, and which is only waiting for development to become a profitable source of revenue. Of coal there is an abundant supply, but only the Takashima mine has been fitted with modern appliances. There are several other coal mines which are only unprofitable because improperly worked, and now it is averred that Prof. Atkinson during a sojourn in the Mitake Mountains of the Koshu Province has discovered another valuable deposit of coal.

MR. NORTH, who was sent by the Natal Government to examine the Newcastle coal-fields, has reported favourably on the quantity and quality of the coal.

ON Friday evening, October 22, previous to distributing at the Manchester Mechanics' Institute the prizes and certificates gained by the students at this year's Science and Arts, Society of Arts, City and Guilds of London Institute, and Union of Lancashire and Cheshire Institute's examinations, Prof. Ayrton delivered an address on Technical Education and on the future of Mechanics' Institutions. Of the two original objects for which Mechanics' Institutions were established fifty years ago, to provide clubs for artisans and places for giving popular scientific lectures, it was shown that the latter had to a great extent been abandoned; also that the mere novel utility of such institutions in furnishing the means for the holding of science and art classes would also be taken away from them when the teaching of elementary science became the duty of our elementary schools. There remained, however, for Mechanics' Institutions a great new field of activity in the teaching of applied science to mechanics, not the teaching of abstract scientific principles and the applications only perhaps afterwards, but the teaching of these scientific principles *through* the apparatus in use in daily life; in fact, that Mechanics' Institutions could well furnish the machinery by means of which numerous technical classes throughout the country which were so much needed could be rapidly established, the money voted by the City and Guilds of London Institute as payment on the results of the technological examinations, together with funds locally subscribed, furnishing the motive power. What the lecturer thought technical teaching should consist of was illustrated by the kind of work now going on at the temporary laboratories of the City Guilds Institute at Finsbury; stress was laid on the fact that there were no distinct students' fees there for laboratory work and for lectures, but that every fee, small as it was, entitled each student to at least two hours' practical work in the laboratories for every one hour of lecture; so that in fact all the 150 students had laboratory work which did not consist in the mere repetition of qualitative lecture experiments, but in the making of accurate quantitative measurements, all bearing as far as possible directly on each student's trade. Of this practical illustrations were given. Prof. Ayrton concluded by warning technical instructors from attempting to follow ordinary college methods of *synthetical* teaching, which, although most valuable for a young lad prepared to spend several years at college, was quite unsuitable for an artisan engaged all day in following his trade. Technical education, he considered, must be distinctly *analytical*—the complete machine as the artisan knew it must be taken at once, and the science developed, so to say, from the machine itself; and that it was men with a good practical knowledge of their trade and with an aptitude for science rather than men versed in science, but with only a mere book knowledge of industries, that were wanted both as candidates for the technological examinations and as students to be trained up as technical instructors.

IN the note on the late Dr. Sparks in NATURE, vol. xxii. p. 591, for Dr. King's "Therapeutics" read Dr. Binz's "Therapeutics."

THE additions to the Zoological Society's Gardens during the past fortnight include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. W. B. Tustin; two Polar Bears (*Ursus maritimus*), an Ivory Gull (*Larus eburneus*) from the Arctic Regions, presented by Mr. Leigh Smith, F.Z.S.; a Crested Porcupine (*Hystrix cristata*) from India, presented by Mr. W. Middleton; three Gaimard's Rat Kangaroos (*Hypsiprymnus gaimardi*) from Australia, presented by Mr. A. B. Gow; a — Brocket (*Cariacus sp. inc.*), a White-bellied Opossum (*Didelphys albiventris*), a Brazilian Hare (*Lepus brasiliensis*) from Quipapà, Pernambuco, a White-bellied Guan (*Ortalida albiventris*), a Black Tortoise (*Testudo carbonaria*) from Garanhuns, presented by Mr. W. A. Forbes, F.Z.S.; a Frigate Bird (*Fregata aquila*) from Fernando de Noronha, presented by the Rev. G. Bayldon; a Yellow-headed Conure (*Conurus jendaya*) from Pernambuco, presented by Mr. C. A. Craven; two American Black-backed Geese (*Sarcidiornis carunculata*) from the Sertoes of Pernambuco, presented by Miss Davis; a White-throated Finch (*Spermophila albogularis*) from Pernambuco, presented by Mr. S. Jones; a Herring Gull (*Larus argentatus*), British, presented by Mr. J. Palmer; a Horrid Rattlesnake (*Crotalus horridus*) from Quipapà, Pernambuco, presented by Mr. H. E. Weaver; a Bonnet Monkey (*Macacus radiatus*) from India, a Black Iguana (*Metopoceros cornutum*) from Galapagos (?), deposited; a Rock Cavy (*Ceredon rupestris*), a Green-winged Trumpeter (*Psophia viridis*), a White-bellied Parrot (*Caica leucogaster*), a Red-vented Parrot (*Pionus menstruus*), two Golden-headed Parrakeets (*Brotogerys tui*), two Toco Toucans (*Ramphastos toco*), an Orinoco Goose (*Chenalopex jubata*) from Brazil, a Rufous Pigeon (*Columba rufina*), a Yarrell's Siskin (*Chrysomitris yarrelli*), two Scaly Doves (*Scardafella squamosa*) from Parahyba, three Picazuro Pigeons (*Columba picazuro*), a Black Tanager (*Tachyphonus melaleucus*), a Black-headed Tanager (*Orchesticus ater*), a Passerine Ground Dove (*Chamæpelia passerina*), three Yellow-shouldered Hangnest (*Icterus tibialis*), from Pernambuco, a Brazilian Tanager (*Ramphocœlus brasilius*), a Blue and Black Tanager (*Calliste brasiliensis*) from Bahia, a White-eyebrowed Guan (*Penelope superciliaris*) from Panellas, four Cactus Conures (*Conurus cactorum*), two Banded Tinamous (*Crypturus noctivagus*), seven Tataupa Tinamous (*Crypturus tataupa*) from Garanhuns, a Great-Billed Rhea (*Rhea macrorhyncha*) from Agoas Bellas, Pernambuco, two Orchard Hangnests (*Icterus spurius*), a Baltimore Hangnest (*Icterus baltimore*) from North America, purchased; two Squirrel-like Phalangers (*Belideus sciureus*), born in the Gardens; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. F. W. Manley; a Dunlin (*Tringa cinclus*), a Sanderling (*Calidris arenaria*), British, presented by Mr. Edmund Elliot, M.R.C.S.; a Horned Lizard (*Phrynosoma cornutum*) from Texas, presented by Mr. W. C. Boyd; a Waxwing (*Ampelis garrulus*), European, deposited; a Black Saki (*Pithecia satanas*) from Lower Amazons, a Roseate Spoonbill (*Platalea ajaja*), a Great-billed Rhea (*Rhea macrorhyncha*) from South America, purchased.

OUR ASTRONOMICAL COLUMN

CERASKI'S VARIABLE OF SHORT PERIOD.—It will be seen from a letter which Prof. Pickering, the Director of the Observatory of Harvard College has addressed to us, that, contrary to

the opinion expressed by Dr. Julius Schmidt from his earlier observations, the true period of this notable variable star, instead of being a little less than five days, appears to be a little less than half this interval, otherwise *minima* observed at Harvard College, will not accord with those of May and August observed in Europe.

It is probable that Schwerd observed the star near a *maximum* at meridian transit at Speyer on March 11, 1828, when he estimated its magnitude 6·7, and near a *minimum* at transit on May 12 in the same year, when he rated it only 10m. If we compare the observation of March 11 with that of Dr. Schmidt, who fixed a *minimum* to August 12 at 6h. mean time at Athens, and assume 7662 periods to be included in the interval, we get for the duration of one period 2·49084d., or 2d. 11h. 46·81m., which closely accords with half the period assigned by Schmidt from his own observations and those of Ceraski. This reckoning from August 12·1841 Greenwich mean time, and correcting for the light-equation, will give the following times of geocentric minima observable in this country:—

	h. m.			h. m.	
Oct. 28 ...	9 33	G.M.T.	Nov. 17 ...	7 47	G.M.T.
Nov. 2 ...	9 7	,,	22 ...	7 21	,,
7 ...	8 40	,,	27 ...	6 54	,,
12 ...	8 14	,,	Dec. 2 ...	6 28	,,

And for the times of visible maxima, supposing this phase to occur midway between the minima, we find—

	h. m.			h. m.	
Oct. 29 ...	15 27	G.M.T.	Nov. 18 ...	13 40	G.M.T.
Nov. 3 ...	15 0	,,	23 ...	13 14	,,
8 ...	14 33	,,	28 ...	12 48	,,
13 ...	14 7	,,	Dec. 3 ...	12 21	,,

If S be the sun's longitude, and R the earth's radius-vector, the correction for the light-equation (geocentric—heliocentric) for 1880 may be found from

$$\text{Cor.} = 224{\cdot}0\text{s R. sin } (S + 19^\circ\ 17'{\cdot}4).$$

We have received from Lord Lindsay a circular containing the same information that is given in Prof. Pickering's letter, with the addition of a diagram showing the *Durchmusterung* stars in the vicinity of the variable, which for 1881·0 has R.A. 0h. 51m. 48s., N.P.D. 8° 46′·0.

[Mr. Knott's observation on October 23, received since the above was in type, as compared with Athens August 12, seems to require a somewhat longer period, with minima a half hour or so later than we have computed.]

THE ROTATION OF JUPITER.—In No. 2,342 of the *Astronomische Nachrichten* (to which we refer for numerical details) Dr. Julius Schmidt has a communication wherein he finds, from observations of the red spot upon the disk of Jupiter by himself and others in 1879–80, an interval of 9h. 55m. 34·4s. for the time of the planet's rotation upon its axis, a result that he considers may be adopted until the observations generally have attained a greater degree of precision than they appear to possess at present. With due care and practice, however, he believes that such observations will be found to admit of much greater accuracy, and illustrates this by his own experience at Athens in the present year. In the same communication he also discusses observations of a dark oval spot (a more favourable object than any used by Airy and Mädler) during 104 rotations in 1862; these observations give 9h. 55m. 25·68s. for the period of rotation, a result closely agreeing with those of 1834–35.

CHEMICAL NOTES

A NEW method of preparing acetylene is described by Dr. W. Suida in *Wien. Akad. Ber.* The method consists in heating iodoform and mercury ethide in sealed tubes to 120°; the products of the reaction are acetylene, ethylene, ethylic iodide, and mercury ethyliodide.

THE same *Berichte* contains a paper by Herr v. Dumreicher on the relative stabilities of nitrous and nitric oxides, and of nitrous and nitric oxides when acted on stannous chloride. Nitrous oxide is not reduced even at 100°; nitrous acid is reduced to nitrous oxide; nitric oxide and nitric acid are reduced to hydroxylamine, and subsequently to ammonia. The reaction may be applied to the estimation of nitric acid.

IN the *Proceedings* of the Academy of Rome Signor Cossa communicates the results of experiments on didymium tungstate: he has determined the specific heat of this salt to be 0·0831—temperaturature limits are not given. Taking the atomic heat of tungsten as 6·4, and that of oxgen as 4, this result points rather to the formula for didymium tungstate, $DiWO_4(Di = 98)$, than to that now generally accepted, $Di_2(WO_4)_3(Di = 147)$.

A NEW organo-metallic compound containing the divalent radicle $(CH_2)''$ is described in the *Journal* of the Chemical Society by Sakurai; the formula of the new substance, for which the name *Monomercuric methylene iodide* is proposed is $I(CH_2)HgI$. This is the first known metallic compound containing a *divalent* hydrocarbon radicle.

G. BOUCHARDAT claims, in *Compt. rend.*, to have converted amylene, by successive removals of hydrogen, into cymene. Hitherto attempts to pass, by a simple series of reactions, similar to those by which the passage from one isologous group to another is effected, from the paraffin to the aromatic group of compounds, have not been successful.

ACCORDING to the experiments of Macagno (*Bied. Centralbatt*) the mellowness of old wine is due more to an increase in the amount of glycerine present, than to a decrease in the tannin; there must also be a certain proportion between the amounts of alcohol and tannin, in order that the wine may keep well.

IN the *Annales Chim. Phys.* Berthelot describes an apparatus in which the combination of two gaseous constituents to form a gaseous compound may be conducted, so as to allow of an accurate measurement of the thermal change which accompanies the chemical change.

A DISCUSSION as to the value to be assigned to the atomic weight of antimony is at present being carried on. From analyses of the bromide and other salts, Prof. Cooke of Harvard concludes that the generally-accepted number, 122, is too large, and that 120 is more nearly correct. Herr Schneider, whose experiments had been criticised by Cooke, replies in the *Journal für Pract. Chem.* He sharply criticises Cooke's methods, gives the details of new experiments, and asserts strongly that 122 is much more nearly correct than 120.

No results of special importance have lately been published regarding the densities of the vapours of the halogen elements. An objection made by Pettersson and Ekstrand to V. Meyer's method, viz. that solid bodies condense air on their surface, which air they again give up when strongly heated, has been shown by Meyer, in the last number of the Berlin *Berichte*, to have no weight against his experiments.

Two important papers on atmospheric ozone have been published in the *Berichte* by E. Schöne. This observer, who has given much careful study to the subject of ozone, says that the smell of ozonised oxygen does not at all resemble the peculiar odour noticed after a lightning flash. The true smell of ozone is, however, frequently noticeable in ordinary air, and coming from the clothes of persons who may enter a room from the open air in winter. The ordinary potassium iodine papers are valueless as ozone measurers, according to Schöne. A small amount of ozone in moist air produces a greater depth of colour on these papers than a larger amount of ozone in dry air. The humidity of the air and the hygroscopic character of the material from which the paper is made therefore largely influence the depth of colour produced. It has been supposed that much ozone is produced in the neighbourhood of waterfalls, but the increased depth of colour of the potassium iodide papers is only due, says Schöne, to the great humidity of the air. Schönbein's "ozonometer" serves as a very rough hygrometer. Paper coated with thallous hydrate is recommended as a measurer of the relative amount of "oxidising principle" in the air: the paper is coloured brown—owing to production of thallic oxide—by ozone or hydrogen peroxide. A table is given showing the variations in "oxidising principle" during 1879. The general conclusions are briefly these:—1. The papers are coloured more deeply during the day than during the night; this difference is more apparent during the long days of the year. 2. Increased wind-force causes increased coloration, because a greater amount of oxidising substance is brought in contact with the paper during the time of exposure. 3. Cloudiness and rain especially influence the coloration; the heavier the rain the smaller the coloration of the paper. Direct determinations of hydrogen peroxide have shown that when the thallium papers are much coloured this compound is present in the atmosphere in comparatively large quantity. Herr Schöne regards the actual existence of ozone in the atmosphere as at present an open question.

MR. A. VILLIERS publishes in the September number of *Annales Chim. Phys.* a lengthy and important paper on the

conditions of equilibrium of mixtures of alcohols and mineral acids. He considers in detail the velocity, and limits of etherification of the more important mineral acids, and arrives at many valuable results.

GEOGRAPHICAL NOTES

AT the last meeting of the Berlin Geographical Society news was received through a German trade house in Tangier that Dr. Lenz had reached Timbuctoo, and that he hoped to be at St. Louis, in Senegal, in the month of July. If this is correct Dr. Lenz has made a rapid journey in this direction, as he only left Tangier on December 22 last. Caillé, however, in 1828, travelled from Timbuctoo to Fez in four months. The last letter received from him by the Society was from Tenduf, in the beginning of May, twenty days' journey from Timbuctoo.

THE *Zeitschrift* of the Berlin Geographical Society, Nos. 88, 89, has a valuable map by Herr Richard Kiepert, showing the work done in Angola in 1876 by Dr. H. von Barth in the region of the Bengo and Lucalla, and of Herr Otto Schütt in 1877–79 on the Lower Quanza. Dr. von Möllendorff discusses the methods of transcribing Chinese geographical names, and concludes that the Pekin form of the Guan-hua, or so-called Mandarin dialect, would be best for general purposes. But Dr. von Möllendorff asks whether, while selecting this form generally, it is advisable to make exceptions in certain cases. Such names, for example, as already exist in familiar forms might be excepted, as Pekin, Canton, Hongkong, Swatow, &c. With other names, especially for special maps, a change from the uniform method of writing might be adopted. Maps of districts for the use of travellers would evidently be of greatest service when the local forms of names were given. Perhaps the Guan-hua might be used for the names of great towns, large rivers, and mountains, while smaller places might have the local forms of their names. For a map of the whole of China, or of the greater part of it, containing little more than the district towns, evidently the Guan-hua would be the preferable form. In books it would perhaps be best to give both forms. It is, no doubt, high time that some attempt at uniformity should be made, but the difficulty is by no means easy of solution, owing partly to the letters of the alphabet not being sounded uniformly in all European languages. Herr von Möllendorff instances the absurdity of the present want of system by the ways in which the Chinese name of the Yellow River is spelled. These are confusing enough, but what will he say when he sees "Houan Hé" (for Hwang-ho) at the head of the interesting communication just received from Col. Prejevalsky? We cannot entirely concur in Herr von Möllendorff's definition of "Kwan-hwa," popularly translated "Mandarin dialect," and he himself makes the orthographical jumble much worse by writing "Guan-hua," which we should imagine few sinologues would attempt to defend. The vexed question, however, may find a solution before long in an unexpected quarter, for the Statistical Department of the Chinese Maritime Customs at Shanghai, we believe, have under consideration a system of spelling for adoption in their reports and other publications, and this, if adopted, will probably come by degrees into general use. Dr. Hildebrandt gives an account of a visit he made to the Amber Mountains in the north of Madagascar; Herr K. Himy continues his elaborate paper on the region around the Kara-Kul, and much of the number is occupied with the journal in North Africa of the late Dr. Erwin von Bary.

THE new number of the Lyons Geographical Society's *Bulletin* contains several items of interest. M. Morice's paper on French Cochin-China is published with a sketch map, followed by some notes by the Abbé Desgodins on the hydrography and orography of Tibet, and a communication by the Abbé Faure on Potosi in Bolivia. Among the other contents are Père Brucker's notes on the geographical positions in Eastern Turkistan and Jungaria determined in 1876 by two Jesuit missionaries, and the report on Col. Flatters' explorations in the Central Sahara last spring.

M. VENUIKOFF has just published at Geneva an historical sketch of the geographical discoveries made in Asiatic Russia from the most remote times to our own days, illustrated by Perthes' map of North and Central Asia.

THE China Inland Mission have been informed by Mr. Samuel Clarke, one of their agents in the Chinese province of Szechuen, that, in company with Mr. Mollman, of the British and Foreign Bible Society, he lately made a journey from Chungking, on the Upper Yangtsze-kiang, to Chêngtu-fu, the capital of the province, on which he travelled by unfrequented roads, where, so far as he could learn, no foreigner had ever been seen before; several previously unvisited towns were also entered. Mr. Clarke calls especial attention to the commercial activity prevailing along his route, and the frequency with which markets were held.

THE Asiatic Society of Bengal have just published, as an extra part of their *Journal*, a "Vocabulary of the Language of Eastern Turkistan," by the late Mr. R. B. Shaw, the well-known traveller, supplemented by two Turki vocabularies of birds and plants by Mr. J. Scully, lately on special duty at Kashgar.

FROM the Vienna *Allgemeine Zeitung* we gather that Dr. Emil Holub contemplates undertaking another lengthened journey in Central South Africa, provided that he can obtain the necessary funds. It is estimated that 50,000 florins will be required for the purpose, and it is proposed to raise this sum by a public subscription, the Austrian Geographical Society heading the list.

THE September number of the *Boletin* of the Madrid Geographical Society contains a detailed account of the Marquesas Islands, with map, by D. Ricardo Beltrán of Rózpide.

IT is stated that the *Gulnare*, with Capt. Howgate's expedition, landed at Rittenbank in Greenland, Dr. Pavy and Mr. Clay, whose intention is stated to be to make natural history collections and explore the northern limits of Greenland. This, we believe, is the same M. Pavy (a Frenchman) whose projected polar expedition suddenly collapsed in San Francisco seven years ago.

THE Austrian *Monatsschrift für den Orient* for October contains an article by Prof. Vambéry on the commercial importance of the Upper Oxus, in which he endeavours to show that there, and not on the Lower Oxus, is trade likely to be developed. Dr. Paulitschke gives an interesting sketch of the progress of African exploration during the past seventy years.

ON MAXIMUM AND MINIMUM ENERGY IN VORTEX MOTION[1]

I. A FINITE volume of incompressible inviscid fluid being given, in motion, filling a fixed, simply continuous, rigid boundary, the fact of its being in motion implies molecular rotation, or (as it may be called for brevity) vorticity. Helmholtz's law of conservation of vorticity shows that, whether the boundary be kept fixed as given, or be moved or deformed in any way, and brought back to its given shape and position, there remains in every portion of the fluid which had molecular rotation a definite constant of vorticity; and his formula for calculating energy for any given distribution of vorticity allows us to see that the energy may be varied by the supposed operation on the boundary.

II. The condition for steady motion of an incompressible inviscid fluid filling a finite fixed portion of space (that is to say motion in which the velocity and direction of motion continue unchanged at every point of the space within which the fluid is placed) is that, with given vorticity, the energy is a thorough maximum, or a thorough minimum, or a minimax. The farther condition of *stability* is secured by the consideration of energy alone for any case of steady motion, for which the energy is a thorough maximum or a thorough minimum; because when the boundary is held fixed the energy is of necessity constant. But the mere consideration of energy does not decide the question of stability for any case of steady motion in which the energy is a minimax.

III. It is clear that, commencing with *any* given motion, the energy may be increased indefinitely by properly-designed operation on the boundary (understood that the primitive boundary is returned to). Hence, with given vorticity, there is no thorough maximum of energy in any case. There may also be *complete annulment* of the energy by operation on the boundary (with return to the primitive boundary), as we see by the following illustrations:—

1. The case of two equal, parallel, and oppositely rotating vortex columns terminated perpendicularly by two fixed parallel planes, which, by proper operation on the boundary, may be so

[1] By Sir William Thomson, British Association, Swansea, Section A, Saturday, August 28.

mixed (like two eggs "whipped" together) that, infinitely near to any portion of either, there shall be some of the other.

2. The case of a single Helmholtz ring, reduced by diminution of its aperture to an infinitely long tube coiled within the inclosure.

3. The case of a single vortex column, with two ends on the boundary, bent till its middle meets the boundary; and farther bent and extended, till it is broken into two equal and opposite vortex columns; and then farther dealt with till these two are whipped together to mutual annihilation.

IV. To avoid for the present the extremely difficult general question illustrated (or suggested) by the consideration of such cases, confine ourselves now to two-dimensional motions in a space bounded by two fixed parallel planes and a closed cylindric surface perpendicular to them, subjected to changes of figure (but always truly cylindric and perpendicular to the planes). It is obvious that, with the limitation to two-dimensional motion, the energy cannot be either infinitely small or infinitely great with any given vorticity and given cylindric figure. Hence, under the given conditions, there certainly are at least two stable steady motions. We shall, however, see further that possibly in every case except cases of a narrow, well-defined character, and certainly in many cases, there is an infinite number of stable steady motions.

V. In the present case, clearly, though there are an infinite number of unstable steady motions, there are only two stable steady motions—those of absolute maximum and of absolute minimum energy.

VI. In every steady motion, when the boundary is circular, the stream lines are concentric circles, and the fluid is distributed in co-axial cylindric layers of equal vorticity. In the stable motion of maximum energy, the vorticity is greatest at the axis of the cylinder, and is less and less outwards to the circumference. In the stable motion of minimum energy the vorticity is smallest at the axis, and greater and greater outwards to the circumference. To express the conditions symbolically, let T be the velocity of the fluid at distance r from the axis (understood that the direction of the motion is perpendicular to the direction of r); the vorticity at distance r is—

$$\tfrac{1}{2}\left(\frac{T}{r}+\frac{dT}{dr}\right).$$

If the value of this expression diminishes from $r = 0$ to $r = a$, the motion is stable, and of maximum energy. If it increases from $r = 0$ to $r = a$ the motion is stable and of minimum energy. If it increases and diminishes, or diminishes and increases, as r increases continuously, the motion is unstable.

VII. As a simplest subcase, let the vorticity be uniform through a given portion of the whole fluid, and zero through the remainder. In the stable motion of greatest energy, the portion of fluid having vorticity will be in the shape of a circular cylinder rotating like a solid round its own axis, coinciding with the axis of the inclosure; and the remainder of the fluid will revolve irrotationally around it, so as to fulfil the condition of no finite slip at the cylindric interface between the rotational and irrotational portions of the fluid. The expression for this motion in symbols is:—

$$T = \zeta r \text{ from } r = 0 \text{ to } r = b;$$
$$\text{and } T = \frac{\zeta b^2}{r} \text{ from } r = b \text{ to } r = a.$$

VIII. In the stable motion of minimum energy the rotational portion of the fluid is in the shape of a cylindric shell, inclosing the irrotational remainder, which in this case is at rest. The symbolical expression for this motion is—

$$T = 0, \text{ when } r < \sqrt{(a^2 - b^2)} \text{ and } T = \zeta\left(r - \frac{a^2 - b^2}{r}\right),$$
$$\text{when } r > \sqrt{(a^2 - b^2)}.$$

IX. Let now the liquid be given in the configuration VII. of greatest energy, and let the cylindrical boundary be a sheet of a real elastic solid, such as sheet-metal with the kind of dereliction from perfectness of elasticity which real elastic solids present; that is to say, let its shape when at rest be a function of the stress applied to it, but let there be a resistance to change of shape depending on the velocity of the change. Let the unstressed shape be truly circular, and let it be capable of slight deformations from the circular figure in cross section, but let it always remain truly cylindrical. Let now the cylindric boundary be slightly deformed and left to itself, and held so as to prevent it from being carried round by the fluid. The central vortex column is set into vibration in such a manner that longer and shorter waves travel round it with less and greater angular velocity.[1] These waves cause corresponding waves of corrugation to travel round the cylindric bounding sheet, by which energy is consumed, and moment of momentum taken out of the fluid. Let this process go on until a certain quantity of moment of momentum has been stopped from the fluid, and now let the canister run round freely in space, and, for simplicity, suppose its material to be devoid of inertia. The whole moment of momentum is initially—

$$\pi\zeta b^2(a^2 - \tfrac{1}{2}b^2);$$

It is now

$$\pi\zeta b^2(a^2 - \tfrac{1}{2}b^2) - M,$$

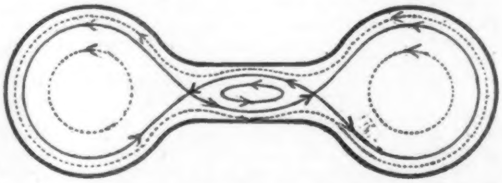
and continues constantly of this amount as long as the boundary is left free in space. The consumption of energy still goes on, and the way in which it goes on is this: the waves of shorter length are indefinitely multiplied and exalted till their crests run out into fine laminæ of liquid, and those of greater length are abated. Thus a certain portion of the irrotationally revolving water becomes mingled with the central vortex column. The process goes on until what may be called a vortex sponge is formed; a mixture homogeneous on a large scale, but consisting of portions of rotational and irrotational fluid, more and more finely mixed together as time advances. The mixture is, as indicated above, altogether analogous to the mixture of the substances of two eggs whipped together in the well-known culinary operation. Let b' be the radius of the cylindric vortex sponge, b being as before the radius of the original vortex column—

$$\tfrac{1}{2}b'^2 = \tfrac{1}{2}b^2 + \frac{M}{\zeta b^2}.$$

X. Once more, hold the cylindric case from going round in space, and continue holding it until some more moment of momentum is stopped from the fluid. Then leave it to itself again. The vortex sponge will swell by the mingling with it of an additional portion of irrotational liquid. Continue this process until the sponge occupies the whole inclosure.

After that continue the process further, and the result will be that each time the containing canister is allowed to go round freely in space, the fluid will tend to a condition in which a certain portion of the original vortex core gets filtered into a position next to the boundary, and the fluid within it tends to a more and more nearly uniform mixture of vortex with irrotational fluid. This central vortex-sponge, on repetition of the process of preventing the canister from going round, and again leaving it free to go round, becomes more and more nearly irrotational fluid, and the outer belt of pure vortex becomes thicker and thicker. The final condition towards which the whole tends is a belt constituted of the original vortex core now next the boundary; and the fluid which originally revolved irrotationally round it now placed at rest within it, being the condition (VIII. above) of absolute minimum energy. Begin once more with the condition (VII. above) of absolute maximum energy, and leave the fluid to itself, whether with the canister free to go round sometimes, or always held fixed, provided only it is ultimately held from going round in space; the ultimate condition is always the same, viz., the condition (VIII.) of absolute minimum energy.

XI. That there may be an infinite number of configurations



of stable motions, each of them having the energy of a thorough minimum as said in IV. above, we see, by considering the case, in which the cylindric boundary of the containing canister consists of two wide portions communicating by a narrow passage, as shown in the sketch. If such a canister be completely filled with irrotationally moving fluid of uniform vorticity, the stream-lines must be something like those indicated in the sketch.

[1] See *Proceedings* of the Royal Society of Edinburgh for 1880, or *Philosophical Magazine* for 1880; Vibrations of a Columnar Vortex: Wm. Thomson.

"Hence if a small proportion of the whole fluid is irrotational, it is clear that there may be a minimum energy, and therefore a stable configuration of motion with the whole of this in one of the wide parts of the canister; or the whole in the other; or any proportion in one and the rest in the other; or a small portion in the elliptic whirl in the connecting canal, and the rest divided in any proportion between the two wide parts of the canister.

ON THE SPECTRA OF THE COMPOUNDS OF CARBON WITH HYDROGEN AND NITROGEN

MESSRS. LIVEING AND DEWAR have made a long series of observations on this subject, of which the following is a brief abstract[1] by the authors:—

The first experiments were made with a De Meritens dynamo-electric machine, arranged for high tension, giving an alternating current capable of producing an arc between carbon poles in air of from 8 to 10 millims. in length. The carbon poles used had been previously purified by prolonged heating in a current of chlorine.

The arc was taken in different gases inside a small glass globe about 60 millims. in diameter, blown in the middle of a tube. The two ends of the tube were closed with dry corks, through which were passed (1) the carbons inserted through two pieces of narrow glass tubing; (2) two other glass tubes through which currents of the gases experimented with were sent.

The arc taken in a globe of air gave a tolerably bright continuous spectrum, above which the green and blue hydrocarbon bands were seen, also the seven bands in the indigo (wave-lengths 4,600 to 4,502, Watts) as in the flame of cyanogen, and much more brightly the six bands in the violet (wave-lengths 4,220 to 4,158, Watts) and five ultra-violet.

Carbonic acid gas, hydrogen, nitrogen, chlorine, carbonic oxide, nitric oxide, and ammonia were then successively substituted for air in the globe, with the result that in carbonic acid, hydrogen, chlorine, and carbonic oxide, the above-mentioned bands in the indigo, violet, and ultra-violet died away, while in nitrogen, nitric oxide, and ammonia, they were always well seen.

These different gases were used in order to eliminate to a large extent the influence of electric conductivity on the character of the spectrum; and the green and blue hydrocarbon bands were seen, more or less, in all of them.

Next observations were made of the spectra of flames of sundry compounds of carbon.

In the flame of cyanogen, prepared from well-dried mercury cyanide, passed over phosphoric anhydride inserted in the same tube, and burnt from a platinum jet fused into the end of the tube, the hydrocarbon bands were almost entirely absent, as Plücker and Hittorf had found; only the brightest green band was seen, and that faintly. The indigo, violet, and ultra-violet bands, on the other hand, were well developed.

These three sets of bands in the indigo, violet, and ultra-violet are in the sequel referred to as the "cyanogen bands," though it is possible that they may be producible by other compounds of carbon with nitrogen.

The flame of hydrocyanic acid burning in air showed very much the same as that of cyanogen.

In the flame of a mixture of purified hydrogen and carbon disulphide no hydrocarbon bands at all could be detected.

Nor could they be detected in the flame of a mixture of carbonic oxide and hydrogen burnt in air.

When a mixture of hydrogen or of carbonic oxide with carbon tetrachloride vapour was burnt, hydrocarbon bands made their appearance, but were weak.

On the other hand, chloroform, when mixed with hydrogen, gave, when burnt in air, the hydrocarbon bands very strongly.

On a review of the whole series of observations, certain points stand out plainly. In the first place, the indigo, violet, and ultra-violet bands, characteristic of the flame of cyanogen, are conspicuous in the arc taken in an atmosphere of nitrogen, air, nitric oxide, or ammonia, and they disappear almost, if not quite, when the arc is taken in a non-nitrogenous atmosphere of hydrogen, carbonic oxide, carbonic acid, or chlorine. These same bands are seen brightly in the flames of cyanogen and hydrocyanic acid, but are not seen in those of hydrocarbons, carbonic oxide, or carbon disulphide. The conclusion seems irresistible that they belong to cyanogen; and this conclusion does not seem to be at all invalidated by the fact that they are seen weakly, or by flashes, in the arc or spark taken in gases supposed free from nitrogen by reason of the extreme difficulty of removing the last traces of air. They are never, in such a case, the principal or prominent part of the spectrum, and in a continuous experiment they are seen to fade out in proportion as the nitrogen is removed. This conclusion is strengthened by the observations that cyanogen (or hydrocyanic acid) is generated in the arc in atmospheric air in large quantity.

[1] For fuller details see *Proc.* R.S., xxx. pp. 152, 494.

In the next place, the green and blue bands, characteristic of the hydrocarbon flame, seem to be always present in the arc, whatever the atmosphere. This is what we should expect if they be due, as Ångström and Thalèn suppose, to acetylene; for the carbon electrodes always contain, even when they have been long heated in chlorine, a notable quantity of hydrogen. In the flames of carbon compounds they by no means always appear; indeed it is only in those of hydrocarbons or their derivatives that they are well seen. Carbonic oxide and carbon disulphide, even when mixed with hydrogen, do not show them; and if seen in the flames of cyanogen, hydrocyanic acid, and carbon tetrachloride mixed with hydrogen, they are faint, and do not form a principal or prominent part of the spectrum. This is all consistent with the supposition of Ångström and Thalèn. The fact that the bands are not produced even in the presence of hydrogen, when it is not present in the flame in the form of a compound with carbon, is very significant; for we know that acetylene is present, and can easily be extracted from the flame of any hydrocarbon, and that it is formed as a proximate product of decomposition of hydrocarbon by the electric discharge, but we have no evidence that it is producible as a product of direct combination of carbon with hydrogen at the comparatively low temperature of the flames described.

The hydrocarbon bands are best developed in the blowpipe flame, that is under conditions which appear, at first sight, unfavourable to the existence of acetylene in the flame. However, by the use of a Deville's tube, acetylene may readily be withdrawn from the interior of such a flame, and from that part of it which shows the hydrocarbon bands most brightly.

The question as to whether these bands are due to carbon itself or to a compound of carbon with hydrogen, has been somewhat simplified by the observations of Watts, Salet, and others on the spectrum of carbonic oxide. It can hardly be doubted now that that compound has its own spectrum quite distinct from the hydrocarbon flame spectrum. The mere presence of the latter spectrum feebly developed in the electric discharge in compounds of carbon supposed to contain no hydrogen, weighs very little against the series of observations which connect this spectrum directly with hydrocarbons.

In the next place, it appears conclusively from the experiments, that the development of violet bands of cyanogen, or the less refrangible hydrocarbon bands, is not a matter of temperature only. For the appearance of the hydrogen lines C and F, observed by the authors in the arc taken in hydrogen, indicates a temperature far higher than that of any flame. Yet the violet bands are not seen in hydrogen at that temperature, while the green bands are well developed. The violet bands are, nevertheless, seen equally well at the different temperatures of the flame, arc, and spark, provided cyanogen be the compound under observation in the flame, and nitrogen and carbon are present together at the higher temperatures of the arc and spark.

The accompanying diagram (Fig. 1) shows approximately the relative position of the bands in that part of the spectrum of the flame of cyanogen fed with a jet of oxygen which is more refrangible than the Fraunhofer line F. Only those bands which are less refrangible than the solar line L have been before described, but photographs show two shaded bands slightly less refrangible than the solar line N accompanied by a very broad diffuse band of less intensity on the more refrangible side of N; also a strong shaded band, which appears to be absolutely coincident with the remarkable shaded band in the solar spectrum, which has been designated by the letter P; and near this, on the less refrangible side, a much fainter diffuse band, which also seems to coincide with a part of the solar spectrum sensibly less luminous than the parts on either side of it. This spectrum is remarkably persistent at all temperatures of the flame. Watts found that it did not disappear when the flame was cooled down as much as possible by diluting the cyanogen with carbonic acid; it retains its characters when the cyanogen is burnt in nitric oxide. The flame in the last case must be one

of the hottest known, from the large amount of heat evolved in the decomposition of cyanogen and nitric oxide, namely, 41,000 and 43,300 units respectively. There is in the case of cyanogen, as in the case of so many other substances, a difference in the relative intensities of the different parts of the spectrum of the flame at different temperatures, but no other change of character. In the upper part of the flame where much or all of the cyanogen is oxidised or decomposed the spectrum is continuous, but at the base of the flame where it is issuing from the nozzle the cyanogen bands are always seen both when the cyanogen is burning in oxygen and when it is burning in nitric oxide. This is what we should anticipate, provided intermediate, and not the final, compounds are the active sources of the banded spectrum.

Each of the five sets of bands shown in the diagram is attended on its more refrangible side by a series of rhythmical lines extending to a considerable distance, not shown in the diagram, but easily seen in the photographs.

Coal gas burning in oxygen gives no bands above that near G within the range of the diagram, Fig. 1; but beyond this photographs show a spectrum of a character quite different from that at the less refrangible end, which the authors have traced to be due to water and described elsewhere (*Proc.* R. S., No. 205).

The authors then describe experiments with carbon tetrachloride, conducted with great care and numerous repetitions because of the prominence given to an experiment with this compound by Mr. Lockyer in a recent "Note on the Spectrum of Carbon," and because their results in every case differ from those which he obtained.

The form of sparking tube employed was similar to that used by Salet. This was attached by thick rubber tubing to a straight glass tube of which one half, about 6 inches long, was filled with phosphoric anhydride, and the other half with small fragments of soda-lime to prevent any chlorine from the decomposition of the tetrachloride by the spark from reaching the Sprengel pump. The tetrachloride used had been fractionated until it had a constant boiling point of 77° C. Sufficient of it was introduced into the sparking tube to fill nearly one quarter of the bulb at the end, and the whole interior of the tube thoroughly wetted with it in order to facilitate the removal of the last traces of air.

When the tube containing the tetrachloride had been so far exhausted that little but condensible vapours were pumped out, the bulb was heated so as to fill the apparatus with vapour of tetrachloride, the pump still going, and this was repeated as long as any incondensible gas was extracted. Sparks were then passed through the tube for a short time, the pump still being kept going. After a short time it was unnecessary to keep the pump going, as all the chlorine produced by decomposition of the tetachloride was absorbed by the soda-lime. On now examining the spectrum, no trace of any of the cyanogen bands could be detected, either by the eye or by photography, however the spark might be varied. The violet lines of chlorine described by Salet were more or less visible, coming out brightly when a condenser was used. Several tubes were treated in this way, and many photographs taken, but always with the same result; no trace appeared of either the seven blue, the six violet, the five ultra-violet, or of the still more refrangible bands of the cyanogen flame. It is true that all the photographs showed three lines in the ultra-violet, but these do not at all closely resemble the cyanogen bands, they are not shaded like them. The least refrangible of the three is coincident with the middle maximum in the ultra-violet set of five bands, but the other two do not coincide with other of these maxima. When a condenser is used, these three lines come out with much greater intensity, and two other triplets appear on the more refrangible side, as well as other lines.

The general character of the violet part of the spectrum of the spark in carbon tetrachloride taken without a condenser, but not the exact position to scale of wave-lengths of all the lines, is shown in Fig. 2. Fig. 3 shows the brightest of the additional lines which come out with the use of a condenser. Photographs of sparks taken in hydrochloric acid showed a precisely similar group of ultra-violet lines, so that there is no doubt that the three lines which the photographs show near the place of the ultra-violet cyanogen bands are due to chlorine.

Repeated trials in which the arrangements were varied having shown that pure carbon chloride, if free from nitrogen, does not give any of the bands ascribed to compounds of carbon with nitrogen, the next step was to determine whether the addition of nitrogen would bring them out, and if so what quantity of nitrogen would make them visible. For this purpose the binding of the rubber tube connecting to the pump a sparking tube containing tetrachloride and found to give no cyanogen bands, was loosened, and, after letting in very little air, immediately closed again. On now passing the spark the six violet bands at once appeared, and the seven blue bands also were in a short time well seen.

After trying some other experiments of a similar kind which indicated that a very small quantity of nitrogen was sufficient to develop the cyanogen bands in one of these tubes, a minute fragment of bichromate of ammonia, carefully weighed and wrapped in platinum foil, was introduced into the neck of one of the

sparking tubes containing carbon tetrachloride, the tube connected to the Sprengel pump, and the air removed as before. On examination of the spark with the spectroscope no trace of any cyanogen band could be detected. A pinch-cock was now put on the rubber tube, and the bichromate was heated by a spirit lamp to decomposition (whereby it is resolved into nitrogen, water, and oxide of chromium). On now passing the spark the six violet bands were well seen. There was no change in the condition of the coil or rheotome, so that the spark was of the same character as it had been before when no cyanogen bands were visible, and the change in the spectrum cannot be attributed to any change in the spark. The weight of the bichromate was between ·0005 and ·0006 grm., and the nitrogen this would evolve would fill just about $\frac{1}{15}$ of a cubic centimetre at atmospheric pressure. The tube held 30 cub. centims., so that vapour of carbon tetrachloride when mixed with $\frac{1}{450}$ part of its volume of nitrogen, gives under the action of the electric spark the cyanogen bands distinctly. Other similar experiments confirmed this result.

Similar experiments with carbon bisulphide, benzol, and well-purified naphthaline, gave like results when care was taken to remove air completely.

As Watts laid much stress on the occurrence of the cyanogen bands in the spectrum of the spark taken in carbonic oxide at atmospheric pressure (though they do not appear in carbonic oxide at reduced pressures), as a proof that these bands were due to carbon only, the authors made a series of careful experiments with carbonic oxide at atmospheric pressures.

In the first experiments with this substance the gas was made by the action of sulphuric acid on dried formiate of sodium, and it was found that the cyanogen bands disappeared as air was expelled from the apparatus, reappearing brightly when air, not exceeding $\frac{1}{100}$ of the whole gas in the apparatus, was admitted.

Carbonic oxide was next generated by heating, in a tube of hard glass in an ordinary combustion furnace, a mixture of pure and dry potassium oxalate with one quarter of its weight of quicklime, the mixture having been previously heated for some time so as to expel traces of ammonia. The tube was connected with a Sprengel pump, and the air exhausted before heating the oxalate. The distant end of the tube with the oxalate was then heated, and the whole apparatus filled with carbonic oxide; it was then again exhausted with the pump, refilled by heating more oxalate, and the gas allowed to stream out through the pump for some time. The heat was then lowered, sparks were passed, and the spectrum observed. No trace whatever of the cyanogen bands could be detected, however the spark might be varied. The pump was now set going again, and the pressure of the gas reduced to one inch of mercury, while the spectrum was observed from time to time. Still no trace of the cyanogen bands could be detected. More of the oxalate was next heated, and the observations repeated again and again, always with the same result. The conclusion was that carbonic oxide, if quite free from nitrogen, does not give, at the atmospheric or any less pressure, the cyanogen bands.

From Dr. Watts's account of his experiments, it appeared that he had used carbonic oxide prepared by the action of sulphuric acid on ferrocyanide, and it was probable that it might have been contaminated with nitrogen, or with nitrogenous compounds, from the ferrocyanide. The authors accordingly repeated their experiments with carbonic oxide so prepared, and found that the cyanogen bands were then always distinctly seen.

They have also repeated Ångström and Thalén's experiments with the spark between carbon poles in nitrogen and carbonic acid gas. They observed that in nitrogen the cyanogen bands were plainly visible through a great range of variations of the character of the spark; even the use of a condenser of moderate size did not diminish them. Photographs were taken with and without the use of the condenser, and these showed the violet and ultra-violet cyanogen bands, including those near N and P. The nitrogen was then swept out by a current of carbonic acid gas, and on now passing the spark the cyanogen bands could no longer be detected, and photographs taken as before showed no trace of any of them.

Other experiments showed the sensitiveness of the spectroscopic tests for compounds of carbon with nitrogen, and that all traces of water can hardly be removed from apparatus and reagents which do not admit of being heated red hot.

The first point the authors had before them in these investigations is whether the groups of shaded bands seen in the more refrangible part of the spectrum of a cyanogen flame are due to the vapour of carbon uncombined, or, as they conclude, to a compound of carbon with nitrogen.

Now the evidence that carbon uncombined can take the state of vapour at the temperature of the electric arc is at present very imperfect. Carbon shows at such temperatures only incipient fusion, if so much as that, and that carbon uncombined should be vaporised at the far lower temperature of the flame of cyanogen is so incredible an hypothesis that it ought not to be accepted if the phenomena admit of any other probable explanation. On the other hand it has been shown that cyanogen or hydrocyanic acid is generated in large quantity in the electric arc taken in nitrogen, and Berthelot has shown that hydrocyanic acid is produced by the spark discharge in a mixture of acetylene and nitrogen, so that in the cases in which these bands shine out with the greatest brilliance, namely, the arc in nitrogen and the cyanogen flame, we know that nitrocarbon compounds are present. Further, the authors have shown that these bands fade and disappear in proportion as nitrogen is removed from the arc. Ångström and Thalén had previously shown the same thing with regard to the spark discharge between carbon electrodes; and the conclusion to which they have come would probably have commanded universal assent if it had not been for the fact that these bands had been seen in circumstances where nitrogen was supposed to be absent; but where, in reality, the difficulty of completely eliminating nitrogen, and the extreme sensibility of the spectroscopic test, had been inadequately apprehended.

To clear up the question from this point of view, the experiments last described have been made, and they appear to the authors quite conclusive. Were the evidence less conclusive than it is, it would still be as rash and as illogical to conclude from the appearance of the cyanogen bands in a case where nitrogen was presumed, not proved, to be absent, that they were not due to a compound of carbon with nitrogen, as it would be to deny that the well-known yellow lines were due to sodium, because they had been seen in cases where sodium was supposed to be absent. The argument of the authors is an induction from a very long series of observations which lead up to one conclusion, and hardly admit of any other explanation. But Mr. Lockyer attempts to explain the disappearance of the bands when nitrogen is absent by the statement "that the tension of the current used now brings one set of flutings into prominence, and now another." This is no new observation. It is well known that variations in the discharge produce variations in the relative intensities of different parts of a spectrum. Certain lines of magnesium, cadmium, zinc, and other metals, very brilliant in the spark, are not seen, or are barely seen, at all in the arc. His remark might be applied to the spectra of compounds as well as to those of elements. Variation in the discharge accounts very well for some of the variations of intensity in the bands if they be due to a compound of carbon with nitrogen; it will not, however, account for the fact that the bands, or those of them which have the greatest emissive power, and are best developed by the particular current used, come out on the addition of a minute quantity of nitrogen, when there is every reason to think that no variation of the current occurs.

Much the same may be said with regard to the changes of the spectrum produced by changes of temperature. We cannot infer from any of these changes that the spectrum is not due to a compound. The bands in question are singularly persistent through a great range of temperatures, from the temperature of a cyanogen flame cooled by admixture with carbonic acid gas, as related by Watts (*Phil. Mag*, 1869, p. 258), to that of the spark of an induction coil with condenser.

But again, Mr. Lockyer attempts to get over the difficulties of his case by the supposition that "the sets of carbon flutings represent different molecular groupings of carbon, in addition to that or those which give us the line spectrum."

Now, until independent evidence that carbon can exist at all in the state of vapour uncombined at the temperature of a cyanogen flame can be adduced, and further independent evidence of the existence of different groupings in such vapour, the hypothesis here enunciated is a gratuitous one, so long as any other hypothesis for which independent evidence can be adduced, as is true of the existence of nitrocarbon compounds in the flame, arc, and spark, will sufficiently explain the facts.

The authors have not expressed any opinion whether or no the cyanogen bands are visible in the solar spectrum. The observa-

tion above recorded that there is in the spectrum of cyanogen a strong shaded band coincident with the very characteristic dark shaded band P, strengthens materially the evidence in favour of the existence of these bands in the solar spectrum; the more so as the series of lines at P has far more of the distinctive character of the cyanogen spectrum than any other series in the ultra-violet part of the solar spectrum.

However that may be, they contend against the hypothesis that if present the bands can be due to any vapour of carbon uncombined in the upper cooler region of the chromosphere. One object of their investigations has been to determine the permanence of compounds of non-metallic elements and the sensitiveness of the spectroscopic test in regard to them. It appeared probable that if such compounds existed in the solar atmosphere their presence would be most distinctly revealed in the more refrangible part of the spectrum, and it seems sufficiently clear that the presence of nitrogen in the solar atmosphere may be recognised through cyanogen when free nitrogen might escape detection.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Millard Scholarship in Natural Science lately founded at Trinity College has been awarded for the first time. The successful candidate is Mr. R. Bodey, from the Mining School, Bristol, and from the Royal School of Mines.

At Exeter College the Natural Science Scholarship has been awarded to Mr. B. Spencer, from King's College, London.

According to the report of the Delegates for unattached students, the number of students not attached to any college or hall has increased by twenty during the past year. Seventy unattached students have become members of colleges or halls during the year.

CAMBRIDGE.—The University of Cambridge Commissioners have apparently proposed their final arrangements as regards the University. There are many modifications from the original scheme in the direction of giving more freedom to the University, and on the whole in favour of scientific objects. A general financial board is to manage all University property and expenditure, and to control especially the college contributions. The rating of the colleges for University purposes is modified in the direction of increased fairness. The common University Fund derived from the colleges is to provide for all classes of University teachers, for the salaries of demonstrators, superintendents, and curators, for the erection, maintenance, and furniture of museums, laboratories, libraries, lecture-rooms; and in addition grants of money may be made from it for special work in the way of research, and for investigations in any branch of learning or science connected with the studies of the University. The amount of payments for buildings, and their maintenance, furniture, and apparatus, is not to exceed one-third of the income of the fund in any one year.

Practically speaking, there may be available in each year to the end of 1884, 2,000*l.* a year for these latter purposes and 4,000*l.* for investigators and teachers, and the college payments will rise definitely to 30,000*l.*, of which 10,000*l.* may be used for the purposes of buildings, and 20,000*l.* for teachers of all kinds.

It is no longer sought to force particular professors on particular colleges; the college may, if it prefers, pay the income of a Professorial Fellowship to the common fund. There are to be twenty-nine Professorial Fellowships, not assignable to particular professors, but distributed among the colleges.

The stipends of the Professors, payable by the University, are to have 200*l.* deducted from them if the Professor holds a Professorial Fellowship or a Headship. The stipends of Professors as now proposed are not so unequal as in the first proposed statutes. The payments (subject to the above-mentioned deduction) to the Regius Professor of Physic would be 700*l.*, Professor of Chemistry and the Cavendish Professor of Physics 850*l.* each, Physiology 800*l.*, Pathology 800*l.*, Botany, Zoology, and Woodwardian of Geology 700*l.* each, Anatomy 600*l.* The new Professorships are to be for (1) Physiology, (2) Pathology, (3) Mental Philosophy and Logic. The first two professors are not to undertake the private practice of medicine and surgery. When these shall have been established, the University may establish any other professorships it pleases, or has funds for.

The proposals for readerships are also to be remarkably modified; the minimum number of readers is now twenty. The subjects are to be within the control of the University; the readers are to be appointed as soon as funds can be provided conveniently from the common University Fund or from other sources. Readerships may be suppressed or created, according to the needs of study. The stipend is to be 400*l.* The readers are to be appointed by grace of the Senate on the recommendation of the General Board of Studies now to be created; but in each case the special Board of Studies with which the readership is connected must concur in the appointment, or it will lapse to the Council of the Senate.

University Lecturers (the next grade of teachers) may be college lecturers who throw open their lectures to the University, or they may be other persons approved by the Boards of Studies.

The payment to these lecturers from the University must be not less than 50*l.* The University may also appoint lecturers on subjects not immediately connected with any special Board of Studies, for shorter or longer terms. The separation of the Board of Studies in Physics and Chemistry from that of Biology and Geology is maintained. The constitution of the General Board of Studies is carefully and completely defined; but it is to do such work as the Senate commits to it, and in future a general University budget is to be prepared and submitted to the Senate.

The Cambridge Museums and Lecture Rooms Syndicate find the increase of annual grant from the University from 1,500*l.* to 2,000*l.* a year inadequate, owing especially to new outlay on new departments. They now have a balance of 821*l.* against them; and they ask for an additional 1,000*l.* per annum at once, feeling quite unable otherwise to maintain the museums in moderate efficiency with strict economy.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, September.—Experiments on the compression of air by the direct action of water, by J. P. Frizell.—Experiments on the strength of yellow pine, by R. H. Thurston.—The absolute economy of electric lighting, by R. Briggs.—Note on the artificial production of diamonds by the processes of Despretz, by E. J. Houston.

October.—Motion of viscous fluids, by T. Craig.—The steam yacht *Anthracite* and the Perkins system of high pressure steam, by G. Deane.—Coal gas engineering, by R. Briggs.—Holman's new illustration of cell-formation, by J. M. Child.—Joseph Henry, by A. M. Mayer.

American Naturalist, October.—S. A. Forbes, the food of the darters.—J. C. Russell, on the former extent of the triassic formation of the Atlantic slates.—C. C. Abbott, notes on stone implements found in New Jersey.—S. Lockwood, some noteworthy birds.—W. K. Higley, on the microscopical crystals contained in plants.—The editor's table.—Biology at the American Association at Boston.—Recent literature.—General notes.—Scientific news.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol. xiii. fasc. xvi., July 29.—On a particular univocal correspondence between elements of space with three dimensions, by F. Aschieri.—Case of unproductivity of corn, by G. Cantoni.—On the thermal and luminous phenomena manifested by the Leyden jar at the moment of its discharge, by E. Villari.—Transformation of aspartic acid into fumaric acid, by G. Korner and A. Menozzi.—First case of repeated peritoneal transfusion, with new and happy success, in an oligocitemic insane person, by C. Golgi and A. Raggi.—On the infirmity of Torquato Tasso, by A. Corradi.—Meteorological summary of the year 1879, from meteorological observations at the Brera Observatory, by P. Frisiani.

Rivista Scientifico-Industriale, September 15.—Further experiments with a Crookes' tube, by A. Righi.—Histology of the skin of Teleostean fishes, by A. Batelli.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, October 6.—H. T. Stainton, F.R.S., vice-president, in the chair.—Sir Arthur Scott of Birmingham and Mr. F. E. Robinson were elected as ordinary Members.—Mr. McLachlan stated that last year he had exhibited

specimens of *Anthocoris nemorum*, an hemipterous insect supposed to be damaging the hops grown near Canterbury, but had then expressed his opinion that the insect was not the true culprit, its habits being probably carnivorous. This year he had received from the same correspondent some small larvæ which had been found in the cones, and these he considered were not only the true enemy of the hops, but were also the food of the *Anthocoris*.—Sir Sydney Saunders exhibited a series of apterous females of the new species of *Seteroderma*, adverted to at the previous meeting, and read remarks thereon.—Messrs. Kirby, Fitch, Ralfe, and the Rev. E. N. Gilbert exhibited several varieties of lepidoptera taken in this country and on the Continent, some of which, from the structure of the antennæ, were considered "hermaphrodite" forms.—Mr. Hildebrand Ramsden communicated a note on *Pyrophorus causticus*, a Cuban fire-fly.—Mr. Swinton read two papers entitled Some Experiments on the Variability of Lepidoptera undertaken during the year 1880, and exhibited specimens and figures in illustration.—Mr. Butler communicated a paper entitled Observations on the Lepidopterous Genus *Terias*, with descriptions of hitherto un-named forms from Japan.—Mr. Waterhouse communicated a paper on the Buprestidæ from Madagascar.—Messrs. Kirby, Distant, and McLachlan called the attention of the Society to a method of publishing descriptions of new species pursued by M. André in recent parts of his work on European Hymenoptera. These were not only inserted on the *cover* of his quarterly parts, but even at the end of sheets of advertisements laid loosely between the pages of a part. It was regretted that no other course than that of protest and disapprobation could be applied in the interest of science to such a practice.

Paris

Academy of Sciences, October 18.—M. Wurtz in the chair.—M. Faye presented the *Connaissance des Temps* for 1882 (204th volume), and indicated several improvements, viz., tables giving, for all points of the globe where the next Venus transit will be visible, the instants of all phases of the transit, a table for determining the direction of the meridian from the Pole star, the positions of 300 important stars every ten days, and of ten polar stars daily, and empiric corrections of ephemerides of the moon.—Longitude of the coast of Brazil, by M. Mouchez. A scientific mission from the United States under Messrs. Green and Davis has, with the aid of the Transatlantic cable from Europe, fixed the position of the six points, Para, Pernambuco, Bahia, Rio de Janeiro, Montevideo, and Buenos Ayres; and the results show that the author's figures for the same places, obtained in 1860 and following years, by astronomical and chronometric methods, were nearly exact, the greatest error being 2·34s. (The *Connaissance des Temps* had adopted different numbers, which are shown to be in error 27·4s.) The author's errors being all of the same sign, a mere shifting of the Brazil coast about 2 sec. westwards (nearly 1km.) would make the longitudes exact to a few tenths of a second. He compares the chronometric and astronomical methods, showing that chronometers, in absence of the telegraph, offer the surest and most simple means of determining longitude. The influence of temperature he corrected by means of a simple coefficient.—On the saccharine matters contained in the fruit of the coffee-tree, by M. Boussingault. He analysed some berries (from Brazil) that had been put in alcohol immediately after plucking, also the alcohol. The berry is poor in saccharine pulp compared with cherries and other stone fruit from which alcohol is got (it has 66 per cent. as against 90 for cherries and 95 for prunes). The distillation of the berries of coffee would hardly be lucrative or practicable (as Humboldt imagined).—Order of appearance of the first vessels in the inflorescence of *Mibora verna*, by M. Trécul.—On the resistance of animals of bovine species to splenic fever, and on the preservation of these animals by preventive inoculations, by M. Chauveau. He mentions that, contrarily to what is observed in France, it is in animals of bovine species that anthracoid diseases are more frequently met with in Algeria. He is inquiring what it is that favours the effects of spontaneous infection in the bovine species, so resistant to provoked infection, and hopes soon to be able to furnish the explanation. The preventive effect of inoculation he has proved in eight subjects of bovine species (four of which were Algerian).—On the photophone of Prof. Bell and Mr. Sumner Tainter, by M. Breguet. A drawing is given of the arrangement found most effective. At M. Breguet's place the phenomena have been obtained with the electric light over a distance of 15m. The articulation, though not perfect, was demonstrative.—Spectroscopic studies of the sun at Paris Observatory, by M. Thollon. The sun has entered on a period of activity. M. Thollon gives figures of several striking recent protuberances. He frequently observes protuberances 1′ in height, and has seen several exceeding 2′ and 3′, and one about 8′. Some of them may nearly reach the limits of the corona. He indicates his new method of ascertaining the direction of the solar equator.—Principles of an algebraic calculus which contains, as particular species, the calculus of imaginary quantities and of quaternions, by M. Lipschitz.—On algebraic equations, by M. West.—Vibratory forms of circular pellicles of saposaccharic liquid (second note) by M. Decharme. This refers to the relative position of the nodals of each system. One finds identical laws for vibratory forms of any circular liquid surfaces, and for those of soapy pellicles; only the width of the zones is about six times smaller in the case of the former.—On the presence of cerium in the coal-formation of the valley of Saint-Étienne, by M. Mayençon.—On a very perfect reptile found in the Permian formation, by M. Gaudry. M. Roche found it at Igornay, and has presented it to the Paris Museum. M. Gaudry proposes to call it *Stereorachis dominans*. Its vertebræ are in striking contrast with those of other reptiles in the same bed; the centrums are in one piece, which adheres to the neural arc. Another mark of superiority is that its humerus has, in the distal part, a neuro-arterial canal. The *Stereorachis* was a pretty large carnivore. It has affinities with the Ganocephali and Labyrinthodonts, and perhaps still more with some of the animals in Mr. Cope's group of Pelycosaurians, in North America.—On the existence of a reptile of ophidian type in the *Ostrea columba* strata of the Charentes, by M. Sauvage.

Berlin

Geographical Society, October 9.—President Dr. Nachtigal, who congratulated Dr. Bastian on his return from his two years' exploration.—A letter from Dr. Buchner was read, dated September 27 of last year from Kimbundo. Since then, it has been learned, he has not only reached the residence of Muata Janvo, but has carried his exploration much farther. It is probable that he has gone northwards.—News was received from Dr. Lenz, which we refer to in our Geographical Notes. By the last news Herr Flegel had reached the confluence of the Niger and Binué, and his expedition was doing well.—The German expedition to East Africa was, according to the last news, at Muhatta, with Capt. Ramaeker's Belgian expedition, on the way to Tabora.—Prof. Credner of Halle read a paper on the glaciation of North Germany during the glacial period.

CONTENTS

atory,
. M.
ances.
s seen
them
s his
or.—
icular
ions,
—Vi-
econd
on of
atory
soapy
maller
n the
nçon,
n, by
ented
tereo-
those
iece,
ority
anal.
ities
still
lyco-
le of
s, by

igal,
ears'
Sep-
been
nvo,
able
Dr.
last
and
pedi-
atta,
ora.
n of